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THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

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EDITED BY

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LECTURER ON OBSTETRIC MEDICINE AT THE LEEDS SCHOOL OF MEDICINE, ETC.

VOL. XXXI. JANUARY—JUNE.

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A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOLLOWING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

DISEASES AFFECTING THE SYSTEM GENERALLY.

DROPSY.—Dont be afraid of giving diuretics in renal dropsy, as most practitioners are. You may give digitalis, squill, and bitartrate of potash, either singly or combined, without materially increasing the irritation of the kidneys. You may give the 75th part of a grain of digitaline three times a day. This dose may seem small, but the tenth of a grain will kill a little dog. (Dr. R. Christison, p. 130.)

GOUT.—In all cases of genuine gout, uric acid exists in the blood in abnormal quantities. In rheumatism this is not the case. Test the thing as follows, by what is called the "uric acid thread experiment:" Immerse a single fibre or two of unwashed huckaback linen in a drachm or so of the serum of the blood, previously acidulated by a few drops of acetic acid, and put into a watch-glass. Put it aside, in order to allow spontaneous evaporation, when, if there be an abnormal quantity of uric acid present, the fibres will be covered with the crystals of this body. (Dr. A. B. Garrod, p. 43.)

Envelope the parts affected in piline saturated with the tincture of piper methysticum, and take this medicine in the following form: R. Tinct. pip. meth. ℥ss.; aquæ ℥vj. Take a fourth part of this mixture every four hours. (Dr. E. W. Pritchard, p. 45.)

Gout and Rheumatism.—Attend to *all* the excretions, and not to those of one organ exclusively. Unless fever be present, give bark and hydrochlorate of ammonia, which stimulate all the excretory glands. Try to improve the general health by change of air and exercise, steel, cod-liver oil, and a variety of other means, before giving colchicum, guaiacum, and other medicines of this kind.

Be careful, especially in puerperal cases, to remember that a joint may be affected by a *gonorrhœal taint*, and may closely resemble pure gout or rheumatism. Ask if the child has had mild or severe purulent ophthalmia: if this has been the case, it is very probable that the joint affection of the mother is gonorrhœal. (Dr. Fuller, p. 39.)

Rheumatism.—If the skin be hot and pulse strong, begin with a mixture of sulphate of magnesia and tartarized antimony until the skin becomes relaxed and the pulse reduced; then give the Rochelle salt in drachm doses every two or three hours in the day-time, until the urine becomes alkaline, when it may be gradually suspended. Externally apply a lotion of carb. potass. ℥j., with opium ℥ij. to the pint of water. The urine will become alkaline in about five or seven days, and from that time the case will generally improve. (Dr. Fuller, p. 41.)

Give two-scruple doses of bicarbonate of potash, well diluted, every two hours day and night, till the patient has been free from the affection for two or three days. (Dr. A. B. Garrod, p. 46.)

Lemon-juice is still relied upon extensively, but since it became known that it contained some of the salts of potash, these have been largely prescribed in the acetate, tartrate, or nitrate of potash. Give half-a-drachm of acetate of potash, ten grains of the nitrate of potash, and ten minims of vinum opii diluted with barley-water, two or three times a day. (Dr. Barlow, p. 45.)

Yellow Fever.—Give fifteen minims of spirit of turpentine every three or four hours, combined with a little nitric ether and camphor mixture. As soon as a remission takes place give quinine, and a more nutritious diet. (Mr. J. Laird, p. 37.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Apoplexy.—Dont be led away by the popular opinion that in all cases of apoplexy we must bleed. If the patient be full and plethoric, with a strong heart, you may bleed; but if he have been intemperate, or have organic disease of the heart or arteries, or of a gouty or rheumatic disposition, do not bleed. In the majority of cases bleeding is improper; they may be better treated by purging, shaving the head, keeping it cool, and perhaps blistering it. (Dr. R. B. Todd, p. 73.)

Chorea.—After an aperient, give two grains of the sulphate of zinc in an ounce of infusion of valerian, three times daily; the dose may be increased to sixteen grains, and the tepid shower-bath used daily. If this does not relieve, give a scruple of the saccharine carbonate of iron instead, and if both these fail, try the ammonio-sulphate of copper, or some preparation of arsenic; if one fails, you will generally succeed with another. (Dr. Peacock, p. 444.)

Convulsions *in Children*.—Use warm water injections to a great extent, even in infants, (twenty-four ounces of warm water have been injected into an infant.) The convulsions will often cease on the injection of the first few ounces, but may return again and again, till the bowels have been well washed out. (Dr. R. Molloy, p. 68.)

Delirium Tremens.—You may give chloroform with safety in those cases where stimulants have been suddenly left off. First, give half an ounce of whiskey in the yolk of an egg, with a little sugar and nutmeg, and when well mixed, and just before being taken, add one drachm of chloroform. Give also strong beef-tea, with a chop, and a little porter, according to the case. If the first dose of chloroform does not produce sleep, give a second, a third, and a fourth, and even a fifth and sixth at intervals of an hour or so, according to the violence of the case. The great point is to keep the patient *well up* with food, such as egg and whiskey, or broth, beef-tea, chop and porter, &c.,—all these to be used judiciously. You may begin to give the chloroform in half-drachm doses every three or four hours, till you feel your way with it, and then increase it, and give it if necessary every hour. (Mr. J. Tufnell, p. 82.)

You may administer chloroform by the stomach with safety, but not by inhalation. You may begin with half a drachm in camphor mixture, with a little liquorice powder, and increase this dose according to its effects. One patient took by mistake four and a half drachms with excellent effect, although it would be imprudent to give such a dose. (Dr. Corrigan, p. 374.)

Epilepsy.—Give the cotyledon umbilicus as in the following formula: ℞. Liq. cotyl. umbil. ℥j., ter die sumend. At the same time give purgatives occasionally, and preparations of iron. (Dr. E. H. Sieveking, p. 65.)

Epilepsy and Convulsive Diseases generally.—Avoid all the usual excitements; avoid low postures, deep sleep, and tight collars or cravats. Relieve acidity of the stomach with bicarbonate of potass, also scybala in the colon by enemata. Sometimes a good emetic, as half a drachm of ipecacuanha, will check a convulsion. It is possible that minute doses of strychnia may do good in the undue spinal excitability of epilepsy, in the same way as a little alcohol does in the undue cerebral excitement of delirium tremens. (Dr. Marshall Hall, p. 64.)

Neuralgia.—Apply aconitine ointment over the course of the nerve, consisting of two grains of aconitine to an ounce of lard. Be careful, however, how you trust a patient to use this ointment continuously, as the drug is cumulative, and the effect may become suddenly powerful. (Mr. J. Hilton, p. 346.)

A liniment, composed of equal parts of chloroform and olive oil, will act most beneficially. (Dr. Corrigan, p. 376.)

If in connexion with the rheumatic diathesis, quinine will almost always fail; you must give colchicum or iodide of potassium, rapid benefit generally follows. (Dr. Peacock, p. 452.)

Sciatica.—Apply a blister, the size of a crown piece, over the seat of pain. Remove the cuticle, and sprinkle over the raw surface a

surfaces in contact, which will induce inflammation; this is easy over a bone, but if upon the soft parts of the calf, you must use leather cones, or smooth wooden buttons, inserted into a small slit or button-hole in one of the wet straps, to keep it in situ. At the same time the dilated trunk must be supported: this is best done by Mr. Startin's elastic riband, wound spirally round the thigh, from the knee to the groin. (Mr. H. T. Chapman, pp. 204, 448.)

AFFECTIONS OF THE RESPIRATORY ORGANS.

Bronchocele.—Endeavour to induce and maintain, for a time, inflammatory action in the gland, which may be followed by adhesive and constructive inflammation from effusion and infiltration, ultimately giving rise to absorption from obliteration of nutrient vessels. Select the most prominent part of the tumour, and apply the potassa fusa over a space the size of a florin. In about twelve days the slough will separate,—then apply the caustic again and again, in succession, until the thyroid is exposed by the gradual destruction of the intervening tissues. Next, pass a blunt-pointed grooved director freely into the substance of the enlargement, in various directions, which will give rise to considerable constitutional disturbance; wait then patiently for the result. (Mr. W. Turner, p. 319.)

Catarrh.—Heat a piece of metal in a spirit lamp, and sprinkle a few grains of powdered opium upon it. Let the patient hold his head in the fumes, and make a few forced inhalations. [This will relieve the great pain in the frontal sinuses, which sometimes exists, but we doubt its efficacy in relieving the febrile affection.—Ed.] (Dr. Lombard, p. 110.)

Empyema.—You may inject a solution of iodine into the pleural cavity. The following formula was successful:—Iodinii ℥j.; potassii iodid. ℥ij.; spirit. rectificat. ℥j.; aquæ ℥v. M. Let this injection remain in the pleural cavity a short time, moving the patient about so as to bring it into contact with all the pleural surface, and then let out as much as will flow out. The greater portion, however, will remain in the cavity. (Mr. J. Windsor, p. 105.)

Laryngitis.—Mix three grains of the solid nitrate of silver with one drachm of sugar. Place the powder in a steel pen, which is itself firmly inserted in a quill open at both ends. This little apparatus is then put into the mouth, so that the end of the steel pen shall be on the root of the tongue, then the lips are to be closed round the quill, and the patient is to respire forcibly. By a little practice, the patient will be able to inhale small particles into his windpipe. (Dr. Ebert, p. 104.)

PHTHISIS.—Amongst other things try a new preparation composed of cod-liver oil and quina, this being, in fact, a solution of quina in cod-liver oil. (Mr. Bastick, p. 115.)

In cases of circumscribed induration, administer iodine in every possible way. 1. Internally in the form of solution, and if necessary, by enemata. 2. In the form of vapour, by inhalation, once, twice, or thrice a day. 3. In the form of ointment, by friction over the diseased parts. 4. By iodine baths. (M. Piorry, p. 104.)

Instead of blistering too much, or as a change, paint the chest with a solution of iodine and iodide of potassium, one part of each, in two parts of rectified spirit. (p. 103.)

Irritable Stomach in.—Give hydrocyanic acid and trisnitrate of bismuth, as in the following formula:—Hydrocyanic acid three minims; trisnitrate of bismuth ten grains. Make this into a draught, with mucilage and green mint tea, and give it three times a day. (Dr. Peacock and Dr. Risdon Bennett, p. 104.)

Night Sweats of.—The oxide of zinc and hyosciamus pill (mentioned in our last volume) is perhaps one of the best medicines to check night-perspirations; also, a mixture of tannin, nitric acid and lupulin.

Purging.—Give a mixture of decoct. hæmatoxyli and aqua calcis, which sometimes succeeds when, perhaps, bismuth has failed. (p. 103.)

PNEUMONIA, *Chronic.*—Counter-irritation is of the greatest benefit. Apply one or two blisters, and follow these by croton-oil liniment, or iodine, or a combination of both. If there be no tubercular tendency, you may employ mercury carefully combined with a more tonic plan of treatment. (Dr. R, P. Cotton, p. 91.)

Typhoid.—Take care how you confound pneumonia occurring in the course of typhus, with a pure inflammatory attack. The symptoms may easily lead you astray, but to treat a case of typhoid pneumonia on the antiphlogistic plan would probably be destructive. Too great weight is placed on physical signs in typhoid pneumonia. The successive signs of crepitus, dulness, cessation of vesicular breathing, and its replacement by bronchial respiration, are too often mistaken for the effects of inflammation, whereas the same signs are seen in typhus. They are, in fact, often the symptoms of typhus, and of all other forms of fever, whether these be variola, erysipelas, purulent poisoning of the blood, glanders, scarlatina, or measles. One of the greatest modern improvements of medical practice consists in treating these cases on the plan recommended by Dr. Stokes. Look attentively to the antecedents of a case of this kind when called upon to treat it; examine its history. The editor has been guided by one simple index in most of these cases,—viz., the brown tongue. If the tongue be ever so slightly brown at the back, take care how you deplete.—on the contrary, begin in good time with broth, ammonia, and wine or good brandy. Dont be afraid of the physical signs of pneu-

monia, if the history of the case and the tongue indicate coming typhus. It is truly surprising how these physical signs vanish by the timely exhibition of stimulants. The blood is decomposing, the colouring matter is acting on the tongue, the stimulating properties are disappearing, and the functions of life will soon cease, unless you compensate for a time for this loss of stimulation, by an artificial one, till the poison has been exhausted or eliminated. These views are also fully supported in some of Dr. Todd's late lectures. (Dr. Stokes, p. 1—20.)

Tracheotomy.—Before opening the windpipe, always introduce a hook into one side, so as to keep the part steady. The opening is then made much more easily, and the orifice kept open and prominent for the introduction of the canula. (p. 322.)

AFFECTIONS OF THE DIGESTIVE ORGANS.

Cholera.—The saline injection in the worst cases of collapse has the certain effect of *prolonging* life, if it does no more. In some cases it has proved the means of recovery. With each pint of water mix 64 grains of chloride of sodium, 64 grains of carbonate of soda, and 32 grains of chlorate of potash—about five pints may be prepared, and to the five pints add half a grain of iodine. The whole five pints may be injected if necessary. (Mr. H. Ancell, p. 390).

Dr. Richardson suggests that instead of injecting a large quantity of fluid at once into the vein, a long narrow syphon tube should be fixed in the vein with the other end immersed in saline solution, in order that "the suction power of the circulating organ may gradually take in from the reservoir the required element." His objection to Mr. Ancell's plan is, that the heart may become exhausted by the sudden over-distention. This objection, however, seems to us to be unnecessary, seeing that the solution may be injected as slowly and gradually as the operator sees fit. (Dr. B. W. Richardson, p. 382).

Rouse the sunken innervation of the ganglionic nervous system, and prevent its deeper prostration. Valerianic acid exercises a directly stimulating action on the abdominal ganglia, as ammonia does on the nervous centres and the whole organism. Thus valerianate of ammonia is one of the best stimulants, as it rapidly enters the circulating fluid. It must be given as early as possible. From 3 to 6 scruples will generally subdue the disease. The following formula will answer: ℞. Valerianat. ammon. ℈j.; aquæ destill. ℥iij.; syrup. simpl. ℥ss. Give a tablespoonful of this mixture every quarter of an hour, and when reaction sets in give it seldomer—at the same time rub the skin with hot flannels. (Dr. Ottinger, p. 398).

Colic, *Painters'*.—Give half a drachm of chloroform every two or three hours, and increase this to a drachm, if necessary. It may be given in any liquid, such as water. (Dr. Corrigan, p. 374).

Dyspepsia, *Atonic.*—When accompanied by acidity and foul eructations give chloride of calcium, liquor calcii chloridii, one drachm three times a day, in a little water. (Dr. A. Leared, p. 118.)

Fistula.—Mr. Partridge uses a probe-pointed bistoury with movable sheath made of silver, which slides off to any extent when the instrument is introduced, thus offering a cutting edge of greater or less extent, according to circumstances. (p. 232.)

Fistula-in-Ano.—Wherever a fistula has existed six weeks or two months, you must suppose that an internal opening exists. The tract that leads to it may be tortuous, but you must search for it again and again. You may introduce a bent probe up the fistula, and bring it out at the anus, and then cut out the probe, if you like, with the bistoury. The intervening parts are generally very superficial. (Prof. Syme, p. 215).

Hernia.—The opinion of Sir Astley Cooper, that the inguinal canal is endowed with muscular contraction, and is consequently influenced by tobacco, chloroform, tartar emetic, the warm bath, &c., is probably erroneous. The canal is altogether passive, and the obstacle to the return of a hernia is generally its size. *Ice,* therefore, is a most valuable remedy. (Mr. Clark, p. 213).

Make a small incision over the inguinal canal, and introduce a fine trocar into the sac. Inject a small quantity of the tincture of iodine, and close the wound by means of the twisted suture. When the sac is adherent to the surrounding parts—when the hernia is congenital—when the sac is distended with fluid, or is thickened or cartilaginous, it may be punctured at once with a trocar without previously making an incision. (M. Jobert, p. 210.)

Strangulated.—Never handle an incarcerated or strangulated hernia too much. In seven out of ten cases it will probably be found that omentum and not intestine is the chief constituent of the tumour, which you cannot get back by rough usage. When you have fairly tried the taxis, the warm bath, or ice, and fail, you must at once use chloroform and have recourse to the knife. (Mr. Stanley, p. 212.)

The cough-impulse may be a valuable diagnostic sign. If there be no effect on the hernia by coughing, you may suspect that the stricture is tight. If impulse be felt in the upper part of the neck of the tumour, and not in other parts, the seat of stricture may be accurately determined, being just below the spot where the impulse is felt. (Mr. J. Luke, p. 214.)

Prolapsus Ani.—Smear the whole surface of the protruded bowel with solid nitrate of silver, and then return it. Repeat this once a week or fortnight. This need not be confined to simple prolapsus, but may be used in cases of hemorrhoidal congestion and thickening of the mucous membrane about the verge of the anus. (Mr. E. A. Lloyd, p. 218.)

In Children.—If the child be about two years old, apply a blister to the cleft between the nates, and dress the blistered surface with 1-20th of a grain of strychnia—on the fourth day 1-16th of a grain may be applied on a second blistered surface, the cuticle of course being removed. Five or six days afterwards this may be repeated if necessary. In a child of four years old, $\frac{1}{8}$ of a grain may be used each time. If this fail, apply the actual cautery in three or four places at the junction of the skin with the mucous membrane. This may be repeated in a few weeks, if necessary, but less extensively. Strychnia may do for very mild cases, but the actual cautery is the most certain remedy. (Mr. A. Johnson, p. 215.)

Piles.—Give 2½ grains of blue pill every night, a scruple of extract of taraxacum dissolved in water, three times a day, and an injection, with two grains of sulphate of iron to an ounce of water, to be used every morning. (Mr. Wormald, p. 452.)

The following ointment may be used when the piles are not of very large size. It is from the Pharmacopœia of Guy's Hospital. ℞. Gallarum contrit. ℥ij.; opii (emolliti aquæ cum ℥j.) ℥ss.; liq. plumbi diacet. ℥ij.; adipis ℥j. M. ft. ung. If the piles be large, ulcerated, and painful, apply nitric acid. (p. 219.)

Bleeding from.—Inject daily a solution of sulphate of copper—fifty centigrammes in a glassful of water. (M. Trousseau, p. 219.)

Pyrosis.—Give gallic acid, unless the affection be accompanied by ulceration or organic malignant disease of the stomach or liver. (Dr. Bayes, p. 119.)

Rectum, *Stricture of the.*—May be from simple induration of the coats of the bowels, but it is far more frequently malignant; and although generally very slow in its progress, it always causes the death of the patient in the end. It exists in that part of the rectum where it makes its turn backwards, within two or three inches of the anus, and never beyond the reach of the finger, though it is often supposed to be situated higher, but this is owing to the practitioner mistaking the promontory of the sacrum for a stricture when the bougie is arrested in its progress by coming in contact with it. (Prof. Syme, p. 446.)

Sarcinæ Ventriculi.—Chloride of calcium is perhaps a better remedy to restrain the vomiting than hyposulphite of soda. Give the following draught: ℞. Liquoris calcii chloridi ℥j.; aquæ ℥vij.; sumat haustum talem ter quotidie. (Dr. A. Leared, p. 115.)

Tape-worm.—Give ℈ij. of the etherial extract of the male shield fern in the evening, and follow this in the morning by ℥j. of castor oil. If necessary give another scruple of the extract the next evening, and another dose of castor oil the following morning. These doses may be repeated according to circumstances. (Dr. J. Hughes Bennett, p. 119.)

AFFECTIONS OF THE URINARY ORGANS.

Bladder, *Apparatus for Injecting.*—Adapt a short bit of india-rubber tubing to the end of the catheter before its introduction. After introducing it, the conical nozzle of a common ear-syringe is easily admitted into the other end of the elastic tubing, and the thing is ready for use. This keeps the catheter more steady. It prevents *joggling.* It saves the use of a stopcock, as the tube is compressible, and lastly, it connects any sized catheter with the syringe. (Mr. Wormald, p. 233.)

Diabetes.—You cannot restrict patients exclusively to animal food. Let animal food however predominate, and mix with it such vegetable matter as will not yield much sugar. Give succulent vegetables and well fermented beer. Insist on regular exercise and warm flannel clothing, with sponging and friction of the skin, always remembering that the skin and kidneys are vicarious of each other to a considerable degree. Dont give opium if you can avoid it, but mild mercurials occasionally; ox gall also may be useful, given so as to mix with the food as soon as it has been got out of the stomach. Creasote, half a minim, given at the time of taking food has been very useful. Give also strychnine, quinine, or cascarilla. (Dr. T. Hodgkin, p. 123.)

Address your remedies to the brain, instead of the stomach or kidneys. Apply blisters, cold lotions, and setons to the head and neck. Apply a continuous galvanic current from the nape of the neck to the epigastrium. Use Strongfellow's or Pulvermacher's portable electro-valtaic battery for this purpose. (Dr. Goolden, p. 129.)

Test for Sugar.—Sugar in very small quantities may be detected by treating the fluid with trisacctate of lead, filtering, removing the lead with bicarbonate of soda or potash, and again filtering by which means a colourless urine is produced, giving, on the addition of a few drops of a concentrated solution of potash and heat, a *bright orange colour.* (Dr. A. B. Garrod, p. 401.)

Incontinence of Urine *in the Aged.*—When not owing to diseased prostate, but probably to want of tone in the bladder, give the following: Tinct. ferri sesquichlor ℥ij.; balsami copaibæ ℥j.; strychniæ gr. j.; infusi quassiæ ℥xij. M. fiat mistura cujus sumatur ℥j. ter die. (Mr. J. Z. Lawrence, p. 232.)

In Children.—Give cubebs; to an infant a few grains, and to a child half a teaspoonful, twice or thrice a day. Sometimes it has to be continued for some weeks. (Dr. Deiters, p. 132.)

Make a mixture of equal parts of tinct. nucis vomicæ and tinct. ferri acet. Give from ten to sixteen drops twice every evening; or give one grain of tannin night and morning. (Dr. Blaschko, p. 131).

Lithotomy.—If a patient be relieved by making water, the case is probably one of urinary irritation. If the pain occurs during mic-

turition only, there is probably stricture, but if the pain be slight *before* micturation, increases *during* the process, and becomes severe for a short time *afterwards*, we have probably a case of stone. The success of lithotomy depends in a great measure on the manner in which the prostate is divided. There is one part which cannot bear injury, this is at the base of the gland where it joins the base of the bladder; it is a dense texture, very tough and unyielding, forming a sort of ring around the urethra, endowed with an extraordinary degree of sensibility. Now, if this texture be torn, the patient will die, or if injured, the danger will be in proportion. The different success of different surgeons depends upon this ring being divided sufficiently by some and not by others: therefore, if on introducing your finger to dilate the deep part of the incision, you feel you have not cut enough, pass a straight probe-pointed bistoury along your finger, and, by a gentle sawing motion, carry the incision on to the extent you find necessary, in order to enable you to dilate with facility. (Prof. Syme, p. 428.)

Lithotomy Simplified.—Introduce a grooved staff in the usual manner, and confide it in the usual way to an assistant. Introduce the index finger of the left hand into the rectum, placing its extremity in contact with the staff as it occupies the prostate, and press it firmly against the staff so as to steady it; then, with a sharp-pointed straight knife pierce the perineum in the middle line, about half an inch above the anus, or at such a distance as may appear necessary to avoid dividing the fibres of the external sphincter. Carry the knife steady and firmly on, till it strikes the groove of the staff, the deep sphincter lying between the knife and the directing finger. Then move the point of the knife along the groove of the staff towards the bladder a few lines, and then withdraw it, cutting upwards, so as to leave an external incision of from three-quarters of an inch to one inch and a half, according to the presumed size of the stone. Next introduce a long ball-pointed probe or wire into the bladder, and withdraw the staff. Then grease the fore-finger of the left hand, and pass it with a semi-rotary motion along the probe, through the prostate into the bladder, when the stone will generally come into contact with it, and if not too large, passes at once into the wound on withdrawing the finger, the patient still having power to strain upon the stone and thus facilitate its expulsion. If the aperture through the prostate be too small, dilate it by means of Arnott's hydraulic dilator, or by adding to the finger india-rubber finger gloves, one over the other, so as to thicken the finger to the size required. (Mr. G. Allarton, p. 421.)

To Prevent Hemorrhage after.—There are many objections to plugging the wound. It prevents the flow of urine, which creates much irritation, and on its removal fresh bleeding is often excited. To obviate this, attach to the common tube a conical bag of oiled-silk, about an inch and a half from its inner extremity, pass the tube

into the neck of the bladder, and then fill the oil-silk bag with sponge. The wound may in this way be most effectually plugged, and the escape of urine is secured by means of the tube. (Mr. J. Hilton, p. 433.)

Retention of Urine.—In retention of urine from obstinate stricture, when the rectum is obliged to be punctured, the instrument sometimes slips out. A modification of Mr. Wakley's instrument is now used at Guy's Hospital. It expands by a spring when once in the bladder. (p. 232.)

Seminal Emissions.—When occurring involuntarily at night, give three granules of digitaline, equal to about one grain of powdered digitalis every night. (M. Laroche, p. 232.)

Stricture of the Urethra.—Stricture never occurs in the membranous portion of the urethra. If you divide the urethra into three portions, the prostatic, membranous, and the remaining part which is covered by the corpus spongiosum, it is in the last portion that all strictures exist. It is never necessary therefore to cut beyond the bulbous part of the urethra, as no strictures are beyond the bulb. If you bear these facts in mind, you need not dilate sinuses, which are sure to close when the stricture is removed. A catheter should be introduced into the bladder after the operation, and retained for 48 hours—not less, on account of the risk of extravasation of urine, and not longer, because it is unnecessary and apt to do harm. (Prof. Syme, p. 221.)

Mr. Syme's set of bougies are exceedingly small in size. No. 1 is perhaps smaller than those made anywhere else. The tightest and worst strictures are *anterior to the scrotum*. A stricture in this place is felt like a small pea, somewhat elongated. This is the kind most unmanageable by dilatation, and most successfully and easily treated by division. Pass a very small director (which is here comparatively easily done, but would be very difficult at the *bulb.)* Introduce the point of the knife into the groove of the director *anterior to the stricture*, and push it backwards through it, while the end of the penis is held firmly. Cut *backwards* in stricture anterior to the scrotum, and *forwards* when the stricture is at the *bulb*. You will never meet with a tight stricture *behind the scrotum* along with an extreme contraction in *front* of it. (Prof. Syme, p. 224.)

A practical distinction of the utmost importance arises from the question whether the stricture is *anterior* or *posterior* to the membranous portion of the urethra. The deep perineal fascia lies in the immediate neighbourhood of the membranous portion, and a wound of this fascia, in opening the urethra, would very likely end in urinary infiltration. When, therefore, a stricture is situated in the bulb of the urethra, it may be divided from without, and any urine which escapes is sure to pass out at the external wound. But if the knife passes along the grooved director or sound, and per-

forates the deep perineal fascia, and wounds the urethra as it passes through this part, the urine which escapes may lodge in the wound made in this fascia, and a drop or two infiltrated behind this dense structure may cause violent inflammation and constitutional disturbance. Hence there are two classes of stricture—those in which it is situated in that part of the urethra corresponding to the corpus spongiosum—and those in which it is situated at the anterior part of the membranous portion of the urethra. In the former, you may divide it with impunity; in the latter, there is great danger of causing infiltration. (Mr. Henry Lee, p. 228.)

Testicle, *Enlarged.*—When compression is wanted, use Hutchinson's air compressor. A kind of double nightcap, made of impermeable material, such as Macintosh, or oil-silk, is folded over the testicle, and then, by means of a stop-cock and force pump, air is forced into the bag, by which almost any degree of compression can be produced. Some forms of hydrocele may be treated in this way. (Mr. Hutchinson, p. 234.)

AFFECTIONS OF THE SKIN, &c.

Acne Rosacea.—Smear the following over the face with the finger every night: camphor, a drachm; milk of sulphur, twice as much, or the sulphur sublimatum. Afterwards as much water as will render it sufficiently liquid. The camphor must be powdered by the usual addition of a little spirits of wine. (Mr. E. Wilson, p. 276.)

Apply a minute drop of the solution of the nitrate of mercury in strong nitric acid, by means of a finely-pointed glass brush, on the top of the tubercle, if indolent. (Mr. J. Startin, p. 343.)

Carbuncle and Boils.—*General Treatment.* Give purgatives and alteratives, with stimulants or salines according to the constitutional disturbance. Incisions give great relief, especially when the carbuncle is spreading and painful; but if the pain has subsided, incisions are not required, but are even injurious. (p. 263.)

Apply a solution of the nitrate of mercury in strong nitric acid (hydrarg. ʒj.; acidi nitric., sp. g. 1·50, ʒij.; solve) to one central spot about the size of a shilling or sixpence. It produces an eschar from beneath which the slough separates. For *boils*, the caustic answers even better than the knife: apply a full-sized drop to the apex of the boil. (Mr. J. Startin, pp. 265, 343.)

Erysipelas.—Apply the protosulphate of iron ʒviiss. to ℥xxxv. of water, or 8 parts to 30 of lard. Apply either of these three times a-day to the part, and to a little distance beyond the disease. (M. Velpeau, p. 275.)

M. Debout recommends the following formulæ. *Ointment*:—Sulphate of iron 5 to 10 parts, water 12½ to 25 parts, oil 12½ to 25 parts, lard 70 to 40 parts. *Solution*:—Sulphate of iron 10 to 20 or 40 parts, water 120 to 110 or 90 parts, glycerine 70 parts. (p. 275.)

Nævus.—If a nævus be small, a simple ligature will be sufficient, but if large, *tie the knot over a piece of bougie or other substance,* which can be twisted each day, until the thread cuts its way through the base of the tumour. (Mr. S. Wells, p. 454.)

In moles and nævi of the face, if superficial, apply a solution of the nitrate of mercury in strong nitric acid. (Mr. J. Startin, p. 344.)

Plastic Surgery, *Hints on.*—A transplanted flap of skin will not unite readily to the denuded edges of a defective part, such as an old cicatrix. Even if it did unite, it would become raised and thickened. The whole of the epidermoid surface of the old cicatrix should, therefore, be pared away, so that two raw surfaces may be brought into contact. Before applying the transplanted flap, the pared edges of the defective part should be loosened from their attachments to the extent of two or three lines, by *flat strokes* of the knife. If this be not done, it is probable that the two edges will not be on the same level, the transplanted part will be raised. (Mr. Spencer Wells, p. 253.)

Psoriasis Diffusa.—When in large patches over the flexures of joints, forehead, neck, chest, &c., give a quarter of a pint daily of the decoction of the urtica dioica (common stinging nettle), made by boiling an ounce of the leaves and stems in a quart of water, down to a pint. An extract may be made from this—dose, five grains three times a day. The same medicine is efficacious in inveterate chronic papular eruptions (lichen), and in various forms of vesicular and scaly diseases (eczema, lepra, and psoriasis), especially in cachectic states of the system. (Dr. J. Bullar, p. 239.)

Scabies, *and other Parasitical Diseases of the Human Skin.*—The vapour of benzoin, or benzole, which is prepared by the decomposition of benzoic acid, destroys parasites more surely than almost any other application. It may therefore be used in pityriasis and scabies most effectually. (M. Reynal, p. 276.)

Sores and Wounds, *Sloughing.*—Use chlorine. It is a powerful antiseptic and prevents sloughing. Make a solution of chloride of lime, two drachms of the powder to a pint of cold water. Cover the wound with pads of tow dipped in this solution, and renew them about twice a day. (Mr. T. S. Fletcher, p. 268.)

Ulcer of the Leg *in Elderly People.*—Opium has a wonderful effect as a stimulant in these cases. Give good diet, and a common opium and soap pill at bedtime. Assist by strapping and bandaging if necessary. The soap and opium pill may be given once, twice, or three times a day if necessary, and if other symptoms do not forbid its exhibition. Sometimes the soap and opium purges: in thi case leave off the soap, and give half a grain of extract of opium night and morning, or once a day; increase to one grain if necessary. Opium gives energy to the capillary system of arteries, promotes warmth, and thus secures an equable balance of the circulation. (Mr. F. C. Skey, p. 269.)

Warts.—When these are about the anus and other parts, sprinkle a little calomel over them every two or three days. In children, use the grey powder, and for distant patients whom you seldom see, use the following: Hydrarg. chlorid. gr. viij.; zinci oxyd. gr. viij.; mucilagin. ʒij.; aquæ ʒvj.; Ft. lotio. (Mr. Lloyd, Mr. Wormald, p. 276.)

Whitlow, *Deep-Seated.*—This being an inflammation of the inside of the sheath of the flexor tendons of the fingers, rapidly running into suppuration, make an early, free, and deep incision into the sheath. If you are obliged to do this in the palm of the hand, you may avoid the palmar arch of blood-vessels by attending to the directions given at page 259. (Mr. J. Hamilton, p. 258.)

Wounds, *Incised.*—These often will not unite by first intention, on account of the oozing of blood on their surfaces. This has been remedied by deferring the dressing for a few hours—but it may be accomplished by *pressure* on the *sides* of the wound by means of dossils of lint, taking care to leave the edges free. Dont disturb the dressing for three or four days. When you cannot apply dossils of lint on account of the irregularity of surface, apply a large sponge (Prof. Syme, p. 316.)

AFFECTIONS OF THE BONES AND JOINTS, &c.

Amputation *of the Thigh.*—In the upper part of the thigh the flap operation answers well, but at the lower third the circular, or a modification of it, is preferable. Mr. Syme often modifies the circular operation as follows: form two short flaps of skin by semilunar incisions on the anterior and posterior surfaces—then retract the skin so as to expose two inches of the muscles *above the angle of union of the flaps*, and divide the muscles in front as high as they are exposed, and those behind as low as they are exposed, to compensate for their greater contraction. Take care to retract the muscles an inch and a half or two inches before sawing the bone, as it is not the length of the flaps that prevents the future protrusion of the bone, but *the height at which the bone is divided above the angle of union of the flaps.* (Prof Syme, p. 158.)

Of the Leg.—Do this about an inch and a half below the tuberosity of the tibia. Take the head of the fibula or the tuberosity of the tibia as your guide, and make a short anterior and posterior flap of integument only, entering your knife about a hand's breadth below the bony prominence at the point which is to form the angle of junction of the flaps at one side. Having dissected up the integuments to an inch and a half below the tuberosity of the tibia, divide the muscles behind about an inch lower down, to allow for their retraction, and then saw through the bones as high as the skin has been dissected up. The nicest kind of artificial leg is made of *tin*, with a *wooden* pin—something like an inverted quart-bottle without a bottom. (Prof. Syme, p. 162.)

At the Ankle.—Make a transverse incision across the sole of the foot, from the tip of the external malleolus, or a little posterior to it (rather nearer the posterior than the anterior margin of the bone) to the opposite point on the inner side, which will be rather below the tip of the internal malleolus. If the incision be carried further forward, considerable inconvenience is experienced from the greater depth of the flap, the operator, getting into the hollow of the os calcis, cuts and haggles in striving to clear the prominence of the bone. Another incision is then to be carried across the instep joining the ends of the former. In separating the flap of skin from the os calcis, you must cut *parallel to the bone.* This is of the greatest consequence, as the flap is supplied with blood only by those vessels which run through it *parallel to the surface*—consequently if you are not careful you divide these vessels, and deprive the flap of nourishment. In dressing the wound afterwards *be careful to avoid all pressure,* as this will kill the flap. In this operation dont leave the astragalus, but take it away, as it is very likely to be involved in the disease, and even if not carious then it is very disposed to be so, and you had better remove *all* the bones which are liable to caries, not excepting the os calcis. (Prof. Syme, p. 159.)

Deformed Bones.—In cases of deformity of the bones, whether from accident or disease, perform what is called subcutaneous osteotomy, which is a much less dangerous operation than resection of bone exposed by a large external wound. Make an incision rectangular with the longitudinal axis of the bone, dividing the integuments and periosteum—apply a fluted chisel-drill (two lines in breadth, attached to a centre-bit) in the centre of the incision, and perforate the bone (the tibia, for example,) transversely from within, outwards. The cessation of resistance safely denotes the accomplishment of the perforation—a narrow saw is now introduced into the aperture drilled through the bone, and the greater part of the bone divided. The bone is directly straightened by fracture of the remainder. The normal position need not be restored at once, but can be accomplished during the consolidation of the bone. (M. Langenbeck, p. 170.)

Excision *of the Elbow-Joint.*—Always remember that in caries of the joint, little more than the articular surface is engaged in the disease. It is the ignorance or neglect of this point which has made many cases unsuccessful. The incisions originally practised by Moreau are the best, viz.: a transverse one carried across the back of the articulation, immediately above the olecranon, from the ulnar nerve to the external condyle, and two longitudinal through the extremities of the transverse, so that the incision altogether has the form of the letter H. When the flaps have been raised, free access is gained to the articulation: and when the operation is completed the edges of the transverse incision, if brought accurately together, generally adhere by first intention. Having dissected up the flaps, do not attempt to remove all the articulation at once, as otherwise

MIDWIFERY, AND THE DISEASES OF WOMEN.

Fibrous Tumours *of the Uterus.*—Order a long course of iodine, the following is a good form: ℞. Potassii iodidi gr. i.; syrupi ferri iodidi ℥xx.; aquæ carui ℥ss. M. ter die sumend. (Dr. West, p. 453.)

Give the liq. calcis chlor. from 30 to 50 minims twice a day for months, or give the Kreuznach-water (artifically prepared by Mr. Hooper) ℥j three times a day in warm water, at the same time using an injection for the vagina, of ℥ij to the ℥j of water. Dr. Preiger, however, chiefly depends on baths and fomentations, composed of the Kreuznach-water, and some extraordinary effects have no doubt been produced by him as we have seen. (Dr. E. Rigby, p. 301.)

Flooding *after Delivery.*—Inject cold water into the womb. Use a Weiss's syringe, and introduce the nozzle of the tube an inch or two fairly into the os uteri. (Dr. S. Wray, p. 294.)

Ovarian Dropsy.—First empty the cyst, then introduce a long flexible catheter, and inject a few ounces of the tincture of iodine of the Edinburgh Pharmacopœia, which is double the strength of the English. Close the wound by strapping, and apply pads and bandages. The cases adapted to this operation are the simple single ovarian cysts. (Dr. J. Y. Simpson, Mr. I. B. Brown, pp. 307, 308.)

Polypus Uteri.—Remove large uterine polypi by incision, rather than by ligature. You can always arrest hemorrhage by well plugging the vagina. For excision use a polypus-knife (called a *polyptome.)* First reach the peduncle by the apex of the fore-finger of the right hand introduced along the pubic surface of the vagina. Then push the instrument along this finger, and by hooking down the peduncle it is divided by the knife, which is placed in the concavity of the instrument, something like the lately improved hernia knife. (Dr. J. Y. Simpson, p. 298.)

Tedious Labour.—The tepid douche, used in the way adopted to induce premature labour, may sometimes stimulate the uterus when inactive. You may use the douche for ten minutes or a quarter of an hour at a time. The same means may assist in dilating a rigid os uteri. (Dr. Bourgeois, p. 294.)

MISCELLANEA.

Charcoal.—This body should be perfectly dry before being used. By drying it before the fire, it may be used over and over again. Charcoal has a most surprising power of *rapidly absorbing sloughs.* It hastens decomposition. In infectious diseases, bolsters and mattresses may be made of it. It completely destroys smells of all kinds when placed near their source. (Mr. L. Ormerod, p. 366.)

As a Disinfectant.—As the most powerful disinfectant, it may be used in various ways. It should be perfectly dry. Heat it thoroughly in a covered crucible, with a small hole in its lid to allow any oxidised material which it may contain to escape, taking care not to have the hole sufficiently large to allow the charcoal to undergo

combustion. When thoroughly heated allow it to cool. Next place it in shallow vessels where wanted to deodorise or disinfect. It will lose its powers in a few days, and ought then to be heated again in the same way. (Mr. G. J. Barford, p. 371.)

Chloroform.—This may be given in various diseases and in larger doses than has generally been done. For example: *In painter's colic*, give half a drachm every two or three hours; increase this to a drachm if necessary. *In delirium tremens*, give at first, one drachm increased to two drachms. Combined with opium it may prove still more valuable, but dont give it by *inhalation*. You may give large doses by the stomach, but not by inhalation, in these cases. *In neuralgia*, a liniment composed of equal parts of chloroform and olive oil will act most beneficially. (Dr. Corrigan, p. 374.)

Dont be guided by the circulation, but by the *respiration* in your exhibition of this remedy. Take a folded towel or handkerchief, presenting a pretty large surface, instead of a small bit of lint, or any other apparatus held to the nose, and dont stint the quantity, but get the patient under its influence as rapidly as you can. The moment the breathing is stertorous, cease the administration. Attend also carefully to the *tongue*, open the mouth if the respiration is difficult, seize the tip of the tongue with the artery forceps, and *pull it well forward*. Always give chloroform in the horizontal posture. (Mr. Syme, p. 376.)

Cod-liver Oil, *Test for*.—Sulphuric acid appears to be a perfectly reliable test of the excellency of cod-liver oil. The *lilac-hue* produced by this acid is different from all other colours. Place a small quantity of the oil to be examined on a white tile, or clean writing paper; drop two or three drops of sulphuric acid into the centre, and instantly a number of radiating currents of a delicate and beautiful lilac hue are seen. The goodness of the oil is in proportion to the richness and persistency of this colour, and the rapidity with which it is produced. The colour produced in all other oils, as in seal oil, train oil, sperm oil, and vegetable oils, is entirely free from a purple or violet tinge. (Dr. E. H. Greenhow, p. 110.)

Elaterium.—In giving elaterium, we all know the great sickness which it sometimes causes. *Capsicum* given with it will often enable us to continue its use much longer than we otherwise should be able to do. Give the capsicum in half or one grain doses. (p. 446.)

New Forceps *for Extraction of Bullets, &c.*—These consist in the blades being separate, so that one blade at a time can be introduced into a sinus or cavity, and when the object is secured, the handles can be locked like the midwifery forceps. (p. 454.)

Nitrate of Silver.—Instead of dissolving this salt in water, dissolve it in common nitric ether. The ether acts as a solvent of greasy matter on the skin, and from its volatility, quickly dries, enabling us to apply several coatings of the solution in a short time. The strength may vary, but about eight grains to the ounce will

generally be sufficient. In erysipelas, this will be found an improvement. (Mr. Ward, p. 336.)

Poisoning *by Lead, Mercury, Gold, Silver, or any other Metal*—Place the patient on a wooden bench, in a metallic bathing-tub, isolated from the ground, and filled with water up to his neck. The water is made slightly acid with nitric or hydrochloric acid, if mercury, silver, or gold is in the system; and with sulphuric acid, if lead is suspected. The negative pole of a pile is then brought into contact with the sides of the bathing-tub, and the positive pole placed in the hands of the patient. The work of purification is now in full activity. The electrical current precipitates itself through the body of the sufferer, penetrates into the depth of his bones, pursues in all the tissues every particle of metal, seizes it, restores its primitive form, and chasing it out of the organism, deposits it on the sides of the tub, where it becomes apparent to the naked eye. (MM. Poey and Vergnés, p. 324.)

By Strychnia.—Animal charcoal is a very general antidote in cases of poisoning; it may be conveyed into the stomach in considerable doses mixed with water, by means of the stomach-pump; it combines with the poisonous principles and renders them inert, or, in some cases it may preserve life by enveloping and isolating the poisonous particles, preventing them coming into contact with the absorbents. In experiments on animals, when strychnia was previously mixed with animal charcoal, they were not in the slightest degree affected. It is equally efficacious as an antidote for all the vegetable and animal poisons, as well as for hydrocyanic acid, and in many instances is more successful than the usual remedies employed in poisoning by mineral substances, especially corrosive sublimate and arsenic. (Mr. W. Chippendale, p. 440.)

Polypus of the Nose.—When you are going to remove a polypus, remember that non-malignant diseases of this description are generally very limited in the seat of their origin from within the nose. They never proceed from the *floor*, *septum*, or *external wall*, but *always from the roof*, or that part which is formed by the turbinated plates of the ethmoid bone, so that the growth always *descends* and fills up the lower parts of the nose, even extending into the throat. Never, therefore, attempt to extract it *low down*, but *high up*. Select forceps of a very small size; introduce the instrument gently along the upper passage of the nose, with the blades separated as much as possible, till you come to the neck of the polypus—then compress the handles, and by a combination of traction and torsion in one uniform direction, break through the connecting textures. If the growth does not follow the instrument, try again and again. (Mr. Syme, p. 317.)

Salivation.—In excessive salivation, or when you wish to put a stop to it, give from ℥ss to ℥j. of chlorate of potass per diem. This may also be given in the same dose in cases of ulcero-membranous stomatitis, or in mercurial stomatitis. (MM. Herpin and Blache, p. 336.)

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PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

Art. 1.—ON TYPHOID PNEUMONIA, &c.

By Dr. William Stokes, Regius Professor of Physic in the University of Dublin.

[The remarks of this eminent physician on the various manifestations and complications of fever have appeared to us so important that we have largely availed ourselves of his lectures, and we hope that in some measure we may be the means of spreading the very important views of practice which these lectures point out. There is a class of cases of typhus in which the lung becomes consolidated or congested more or less.]

To these cases the general term of "typhoid pneumonia" has been given. But you will be convinced, when your experience has been enlarged, that under this term many different forms of disease have been classed; and it is very doubtful whether a true pneumonia is ever developed in the course of a typhus fever. You will meet with the physical signs which attend pneumonia; but these, as you must know, are insufficient to establish the existence of the disease; and even these very physical signs are seldom so well marked, so complete, as it were, as in simple inflammation of the lung. Nor, again, do they follow in the regular succession which we find in true pneumonia.

You know that I am not fond of fine-drawn distinctions in disease, especially when these distinctions are based on some anatomical speciality, and do not lead to any differences in our principles of treatment; and we shall arrive at more practical results sooner by reviewing some of the more striking cases, I will not say of typhoid pneumonia, but of the acute asthenic diseases of the lung which tend to consolidate that organ. I mean, when using the term "acute asthenic," to imply a disease which forms more or less rapidly, and is associated with, or secondary to, a condition of the system in which, with fever of some kind, we find signs of debility. We may recognise the following forms:—

1. Congestion, with more or less consolidation in cases of what is called "diffuse inflammation," "erysipelatous inflammation," or, by some, phlebitis.

2. Similar or nearly similar conditions (so far, at least, as we are taught by pathological anatomy) in cases of purulent absorption, with or without manifest phlebitis.

3. The intercurrent disease of the lung in cases of the eruptive fevers, when they are of the low, putrid, or malignant type.

4. Congestions and semi-consolidations of the lung, as intercurrent affections in typhus fever.

5. Analogous conditions arising in the course of the non-maculated or the so-called "typhoid fevers."

6. The disease occurring in connexion with delirium tremens from excess. In such cases, you will often find a group of asthenic local diseases, which are generally seated in the stomach, heart, the bronchial membrane, the parenchyma of the lung, and even the pleura. In some instances we find that the patient has also typhus fever.

7. Rapid, extensive, and complete consolidation of the lung occurring in the course of a typhus fever. In someinstances the patient dies asphyxiated; while, in others, a portion of the pulmonary strictures falls into sphacelus, and death takes place with the symptoms of acute gangrene of the lung.

Such are some of the more prominent cases which have been classed under the head of typhoid pneumonia. There is another of which we have had many examples, and yet I do not wish you to take what I am going to say about it in any other way than in the light of suggestions. The case, as I have said before, is by no means uncommon. The patient is attacked with the usual symptoms of typhus fever, and he comes into hospital after two or three days' illness. There is nothing about him to make one think that his disease will not run the usual course of the epidemic of the day, and we are prepared to expect a fever of at least a fortnight's duration. On admission, he may have no symptom which would call attention to his chest; but, as early in some cases as the beginning of the fourth, and in others of the fifth day, it is discovered that the upper lobe of one lung is solid, or nearly so. The clavicle is quite dull on percussion, so is the scapular spine, and the dulness extends to the line of the mamma. This discovery has been so often made accidentally, that I am sure many of such cases have passed unnoticed, at least, where the attendant is not well informed as to the insidious nature of typhus local diseases, and does not make it a practice and a duty to examine daily, as far as he can, the condition of every organ.

But the most remarkable circumstance in these cases is, that the constitutional disease seems to be cut short. The expression of fever leaves the countenance; the peculiar colour or hue of typhus disappears; the eyes becomes bright and intelligent; the tongue cleans; and the pulse comes down to a natural state. And thus we have seen patients so altered in the course of twenty-four hours, that one had some difficulty in recognising them. All the symptoms of typhus were gone, and nothing remained but the consolidation of the lung.

And this, too, is not attended with any notable suffering. There may be a little cough, some dull pain, or an inability to lie on one side; but that is all. The respiration is scarcely, if at all, accelerated; in fact, it would seem that there was no irritation or excitement of the organ; and the case is another proof of how much less the sufferings in disease are connected with the mechanical than with the vital conditions of organs.

This local disease, too, is generally easily managed. A small blister or two, the application of the tincture of iodine, and the exhibition of a little hydriodate of potass, with or without a tonic, will remove it. Indeed, the cure is often so rapid, that I have thought that our remedies had nothing to do with the result. How are we to look at such cases? That they are not examples of inflammation of the lung is plain; and it appears probable that, if this local disease had not occurred, the patient would have gone through the course of the fever of the day. Does it not seem as if the constitutional disease exhausted itself, as it were, in the production of the local affection, just as, in certain cases of simple variola, we see the fever to subside on the appearance of the pustule? I do not know whether such cases have been observed elsewhere, but of their existence we have had abundant proofs. It is worthy of remark, too, that when when we compare these cases with the ordinary forms of typhus, attended with secondary disease of the lung, the local affection is here developed at an unusually early period; and it may be. that, in the more protracted cases of fevers, the nature of which is to develope local affections, the periods of this development and of the cessation of the fever may also be coincident. We do not, however, find that this is so common as to establish a rule. Let us, assuming that these curious cases were really examples of typhus with a secondary deposit, again compare this with the more ordinary forms of the disease, and we shall find that they want two important characteristics of the longer fevers; one, the successive production or the simultaneous production of various local diseases; and the other, the occurrence of that secondary inflammation or irritation of the parts in which the deposit takes place. That the latter circumstance is one of great weight in relation to the preventing or delaying of crisis, it is impossible to doubt. As to the case of successive or simultaneous productions of local diseases, this, at all events, marks a more severe and complicated disease.

I will not here enter into the wide subject of crisis in fever; yet I may point out to you, as a matter well worthy of investigation, the possibility of the occurrence of crisis by other modes than those which are generally enumerated; thus we may have a crisis without sweating, or diuresis, or hemorrhage, or diarrhœa, but which takes place by an internal and silent change in the condition of an organ, and yet a change which will, or may, itself spontaneously disappear.

Here let me warn you against the error which so many fall into with respect to these various cases of disease of the lung, arising in the course of some form of constitutional disease or fever. They

are usually set down as pneumonia, typhoid pneumonia by some. Now, the name itself would be of little moment if its adoption did not lead to errors in practice. And, although it cannot be affirmed with certainty that in none of these cases is there pneumonia, yet we have good grounds for believing that, in many of them, inflammation, as the term is commonly understood, is either absent from the first, or, if it occurs, that it is only secondary to a special lesion induced by some form of constitutional disease.

It is extremely difficult to present to you any well-defined classification of the various forms of diseases which have been described under the head of typhoid pneumonia, or to draw the line between simple asthenic inflammation of the lungs and those conditions which have been described from an early period under the terms of bilious, putrid, or typhoid pneumonia. And, observe, that when I make use of the term asthenic pneumonia, we refer more to the condition of the general system than to the activity or inactivity of the local disease. For, so far as local inflammatory action is concerned, there is abundant proof that it may originate and proceed with rapidity, and even with vehemence in the very last periods of life, so that the disease may be sthenic *quoad* the local condition, and yet the case itself be asthenic in reference to the general state of the economy. Much of the confusion with regard to this subject has arisen from the circumstance, that too great weight was attached to the presence of certain physical signs, which were taken as always indicating similar conditions. The succession of the signs of crepitus, dulness, cessation of vesicular breathing, and its replacement by bronchial respiration, has been admitted to indicate a simple pneumonia, in which the local disease is the principal pathological condition, and the fever only a secondary one. But it is certain that this train of phenomena, or some modification of them, may occur under exactly opposite circumstances, the local disease being symptomatic of the fever, and not the fever of the local disease. And there is the strongest reason for believing, that even though the mere anatomical condition of the lung in the two cases be similar, yet there is an essential, a vital difference, and that practically we cannot deal with the local disease in the latter case as if it were an original affection. This applies to all those cases of disease with the physical signs of pneumonia, which are secondary to any forms of fever, whether it be typhus or typhoid, whether it be variola or erysipelas, purulent poisoning of the blood, glanders, malignant scarlatina, or malignant measles. In these cases, even though the physical signs accurately correspond with those of the typical pneumonia, which, by the way, is by no means always the case, we must believe that we are dealing with a special condition of parts, and a condition not only special as compared with ordinary pneumonia, but having also another speciality—namely, that which is derived from the parent malady.

In the present state of our knowledge, we cannot declare that any

special anatomico-pathological condition exists by which we can distinguish these secondary diseases one from the other; but we may say this much, that practically they all appear to agree in being indicative of an asthenic state of the system, and that, therefore, the supervention of their physical signs at any period of those various diseases, must not be permitted to divert your attention from the general condition of the patient, or make you proceed to treat a case as one of sthenic pneumonia because it has some, or even all, the physical signs of that condition. Do not suppose that I am taking up your time unnecessarily by insisting on these points, for they lead us directly to deal with one of the greatest, if not the greatest and most wide-spread error in the practice of medicine; namely, the treatment of all local, acute, and febrile diseases as inflammations. Here is a group of acute, local, and febrile diseases, and not only this, but a set of cases exhibiting some or all of the physical phenomena of acute pneumonia; and yet, if we subjected them to the ordinary routine treatment of inflammation, the worst consequences would almost certainly follow. You must learn to look at the antecedents and the accompanying general phenomena of these diseases, and set your face against the adoption of any treatment which is based on the doctrine that they are original inflammations. I believe that the erroneous views to which I allude, are, I am happy to say, every day becoming less and less frequent, (thanks to our improved system of clinical instruction,) and the independent spirit of investigation which now animates so many of our students. Notwithstanding, they are still too often acted upon, and over and over again patients who have enough to contend with as the victims of some fell fever or other constitutional disease are lost, or assisted to their death by the adoption of a local or general anti-phlogistic treatment, in consequence of the physical signs of a pneumonia being discovered; their stimulants are withheld or withdrawn: tartar emetic, or mercury, or even bloodletting is rashly resorted to; and it often happens that, even though the physical signs of the pneumonia are removed or modified, the patient sinks from the combined effect of the original disease and the exhaustion produced by this ignorant and benighted treatment. I do not think that any of you will fall into these or similar errors after what I have so often said; it will, at least, not be my fault if you do.

Let us now consider what we may term the parenchymatous affections of the lung in fever, or, if you will, the typhous disease of the pulmonary structure. It may be stated generally, that whatever be the differences in the various cases of this affection in typhus, the local disease follows the general law of other lesions secondary to the fever; that is to say, it agrees with them in its mode of invasion, its latency in the earlier periods of its development, its frequent and spontaneous retrocession, and lastly, in its pathological effects. It is quite true, that, as compared with the best marked examples of acute sthenic pneumonia, it is not wanting in any of the physical signs of

that disease taken singly, but it is generally different from it in the order or arrangement, as it were, of these physical signs. And, indeed, I think it is an extremely rare circumstance to observe in the course of a typhus fever the rise, progress, and retrocession of a pneumonia which has passed into hepatization, as we so continually see in ordinary cases of the disease. I have already drawn your attention to those curious cases of consolidation of the upper lobe of the lung; now, whether these be genuine examples of an arrested typhus or not it is difficult to say, but their whole history and progress is very different from that of ordinary pneumonia; and I repeat, that there is nothing more rare than to see in the course of a typhus fever that regular succession of phenomena with which Laennec has made us so familiar, as indicating the several successive stages of an acute pneumonia occurring under what may be called its normal conditions.

The most common case is the occurrence, generally at an early period, especially in the maculated forms, and often at a later period in the non-maculated or so-called typhoid fevers, of a well-marked crepitating râle in the lower lobes of one or both lungs; it is generally much more extensive and distinctly marked in one lung than in the other. The amount of dulness is seldom very great, and we find the disease, as it were, to linger, and for days together to show no disposition either to produce solidity of the lung on the one hand, or to proceed to resolution on the other. The disease is often quite latent, and is only recognisible by careful physical examination; and its discovery, as you will readily understand from what I have said before, is sometimes an unfortunate circumstance for the patient. In the present state of our knowledge we must believe this condition of the organ to be either the result of a certain amount of typhous deposit into the lung, or of a special state, an inflammation, if you will, which is, however, under the general law of the fever, partaking of its specific character, and capable of spontaneous retrocession. It is seldom attended by pain or by hemoptysis, and it constantly exists without any important modification of the general symptoms of the case.

The second form of the disease is of a more serious character, and seems to be connected either with an original pyogenic disposition, itself secondary to typhus, or we may suppose that the typhoid deposit undergoes a rapid purulent transformation, having some resemblance to the third stage of pneumonia, as described by Laennec. I have not myself seen a sufficient number of these cases to justify me in speaking very decidedly as to their physical signs; but I think I have seen enough of them to warrant me in believing that the course of the disease is different from that of ordinary suppurative pneumonia. We have not observed the intermediate stage of well-marked hepatization between that which is characterised by the occurrence of early râle on the one hand, and the signs of insterstitial suppuration on the other. The complete dulness and the bronchial respiration which accompany the third stage of pneumonia as described by Laennec we

have not observed, the physical signs being principally a persisting râle passing from a fine into a large crepitus, and semi-dulness on percussion. On dissection, the lung is found soft, friable, of a greyish-red colour, but still very permeable to air, though infiltrated with purulent matter. It is as if the purulent secretion took place coincidently, with or immediately after the first or congestive stage. Some of the patients have had sweatings and a sanguinolent and somewhat sanious expectoration; but we have not hitherto observed the ordinary prune-juice sputa in these cases. I have seen this disease in connexion with purulent deposits in the neck, and posterior mediastinum, but it may occur without the formation of purulent matter in any situation other than the lung; it may supervene in the advanced periods of the case, and at a time when the patient seems about to recover; or it may come on much earlier, and when the skin is thickly covered with the petechial eruption. The last case is the most formidable; though one attended with the greatest danger, the disease is, however, not always fatal; and we have had several cases in which recovery took place. I need not say, that they were all treated upon a tonic and stimulating plan, in addition to which we employed dry cupping and blisters.

The last case of which I shall speak is by far the most formidable of the pulmonary affections of typhus; it is characterised by the sudden, complete, and singularly extensive consolidation of the lung. In the course of twenty-four, or even, sometimes, of twelve hours, the most extensive and complete dulness may be produced in a lung which had been previously free from physical signs, or, at most, had only exhibited some of the ordinary bronchial râles of typhus. We have thus the signs of complete hepatisation, unpreceded by the crepitating râle; the disease begins by consolidation, and then one of two results follows: either the patient dies speedily, generally with loose râles in the opposite lung, combined with tracheal effusion; or, after a day or two, he begins to expectorate a horribly fetid matter, and we discover by the stethoscope that a large cavity has formed in the lung. This is a true gangrenous cavity formed in the very centre of the solidified mass, and the disease has a close pathological analogy to the process of acute mortification which has been described as occurring in some of the worst cases of the typhous disease of the intestinal glands.

Let us now pass in review the circumstances in which these forms of disease occur; for, when we compare them with the ordinary conditions of acute primary pneumonia, we cannot but admit that they indicate a lesion of a very different nature.

In the first place, the physical signs are preceded by fever; and it may not be until many days have elapsed that the symptoms of lung affection, as it were, spontaneously arise. 2ndly, the fever is obviously an essential fever; it may occur with or without petechiæ, and other complications may or may not be present. 3rdly, the disease sets in without any apparent external exciting cause. 4thly, when the puru-

lent form is observed, it appears to be, as it were, the second stage of the affection; and I may here remark, that, on dissection, we rarely, if ever, find what is termed concrete or non-concrete purulent matter. Lastly, the invasion of one form of the affection may be sudden, and the signs of extensive and complete consolidation be among its earliest phenomena. It is in this case too, that, if time be allowed, large eschars forming cavities, which may communicate with the bronchial tubes, are liable to occur.

I may remark here, that in two cases of this rapid consolidation, the gangrenous eschar did not communicate with the bronchial tubes. One was a case of typhus, in a man who had long before suffered from gangrene of the opposite lung; the other occurred in a case of what is termed the erysipelatous or diffuse inflammation.

[In another lecture Dr. Stokes gives us some very interesting comments on the case of a butcher attacked by fever. He considers that many of these butchers are very liable to attacks of typhus in various forms and complications, such as pneumonia and arthritis: their systems are too highly animalized. This patient had an attack of epistaxis.]

Now, what may we suppose is the cause of epistaxis in this case? It is extremely difficult to explain it; but as far as we know, it does not seem traceable to the causes which ordinarily induce this form of hemorrhage. We do not find it attended with symptoms of active determination of blood to the head, with throbbing of the arteries, and the other symptoms of ordinary active epistaxis; it seems rather to be connected with a dissolved or impoverished, or in some way diseased state of the blood. When we compare typhus fever with that form which gets the name of typhoid fever, with respect to the frequency of this symptom, we find it is much more common in the latter than in the former disease; epistaxis is a common symptom in the non-maculated fevers, while it is rather, I would say, a rare symptom in the petechial typhus. You would not expect this from any *à priori* reasoning, but the fact is so; and there are other facts connected with these two forms of fever which appear to show, that in the typhoid fever there is some condition of the blood more analogous to what occurs in ordinary cases of anæmia, or in chlorosis, than in the true typhus fever. We shall see by-and-by, that with respect to the development of murmur of the heart, it is rare in the true typhus fever; while we have seen it over and over again in the typhoid, non-petechial, and the relapsing fevers. Other arguments might be brought forward to prove this; but, when we speak of diseased states of the blood, we are speaking on very uncertain grounds. There are two facts, at all events, which are of importance—one, that in the true petechial typhus epistaxis is rare, while it is not uncommon in the other forms of febrile disease; the other, that cardiac murmur is rare in the true petechial typhus, and not uncommon in convalescence from

the typhoid disease. If you take two reports on fever, one from a British Hospital—an Hospital in London, in Scotland, or one in Ireland,—take for example Dr. Cheyne's reports, or any of our other extensive reports on fever, and compare it with a report on fever from a Parisian Hospital, and you will see how much more frequent epistaxis is in the Continental fever than in ours.

All of you that have been working so diligently in the fever wards must have observed, that since the session began, petechial fever, in its common acceptation, has been a very rare thing indeed with us; almost all our cases have been of a non-petechial character. This is a very interesting point, and the question for us to consider here is, whether this patient has the epidemic of the day, of which epistaxis is a not unusual symptom, or whether, in this case, the hemorrhage may be traced to a morbid condition of the blood, such as occurs in purpura; or whether he has a pyogenic fever; and these are important considerations; or whether, again, he may not have them all mixed up together; and here we perceive one of the greatest advantages of clinical study in the actual observation of disease at the bed-side, as distinguished from the study of disease in books, viz., that in studying from books we are generally led to consider diseases in isolated forms, pictures of particular affections; but, in an Hospital, we find, that in actual and especially in acute affections, isolation is the exception, and combination is the rule; and these combinations of diseases are infinite. This is the reason why a student of six months' or twelve months' standing, who has worked diligently in a clinical ward, is, I believe, in many instances, a better practitioner than the most learned man, who has drawn his knowledge exclusively from reading, however extensive. Do not suppose from this that I want to decry the study of medical works. Far from it; you must both read and observe; but it is in the clinical ward that you will learn what has not yet been taught, but which must be taught—which has not, at least, yet been taught properly in works of practical medicine; I mean the combinations and infinite complications of diseases, both local and general, and local with general.

I was observing just now, in the ward, in the case of the boy who had the pulmonary lesion, (I will not call it pneumonia of the lung,) with a low typhoid fever, (he is under the care of Mr. Daly,) how well this case illustrates the advantage of clinical study. If you take works upon disease of the heart, you find that it is assumed by almost every writer, that the first sound of the heart and the second sound of the heart are to be easily distinguished from each other. There are some persons who, if you were to say to them, in any given case, "I think that I have had considerable difficulty in saying which was the first and which the second sound of the heart," would set you down as very deficient indeed, as one that had not been properly taught, and did not know his business. But the fact is, that there are many cases in which at first it is very difficult indeed to say which is the first and

which the second sound of the heart. There are cases in which the most experienced man will require repeated observation before he can make up his mind on the point.

It has happened to me over and over again, that, after I thought I had made up my mind by examining at one part of the heart, when I changed the stethoscope an inch or two, I was again thrown into doubt.

I mention this to show you how diffident we should be in our opinions upon these subjects, how slow we should be to condemn men because they do not come up to the mark laid down in books. The truth, in fact, is, that they go beyond it—that they are wiser than the authors of such books.

There are two cases in which it is often extremely difficult to say which is the first and which the second sound of the heart. One of these is, that triple combination which is so common, especially in private practice, where the patient has chronic bronchitis, a weak and irregular heart, and congestion and enlargement of the liver. This is a very common triple combination, if you speak merely of local diseases. But there is another element very commonly to be found in it, and that is, the gouty element; so that you may have a gouty man with chronic bronchitis, with a weak and irregular heart, and with an enlarged liver. In such a case, it is sometimes extremely difficult to say which is the first and which the second sound of the heart. The two sounds are closely similar one to the other; and the action is so irregular, so uncertain, that you may be often for minutes together with the stethoscope carefully applied, and yet not be able to make up your mind. That is one case. Well, take another—such as that of the boy above stairs.

This boy presents some very curious phenomena; and he illustrates, again, difficulties which you would not anticipate, if you merely depended upon the text-books for a diagnosis of disease of the heart. There are two difficulties here. It is difficult to say whether the murmur which he has belongs to the first or to the second sound. But there is a greater and a still more important difficulty in this case, viz., to determine whether this is an organic murmur or an anæmic murmur; and I am not ashamed to say that my own mind is not made up on the subject. In short, I could not take upon myself to say which it is. It would be very easy to adopt one theory or the other, and to argue in a very specious manner upon it; but I know thoroughly the difficulties of the subject; and I declare to you, that I think, at this moment, it would be hardly possible to say whether this boy has disease of the valves of his heart or not. There is one consideration connected with the case which is drawn, not from physical examination at all, but from the general history of the patient, and it is this, that, while organic murmurs are rare—very rare in the form of disease which he has had—inorganic murmurs are comparatively common.

This is a very strong point. We are here under this difficulty, which you meet with every day in private practice,—that you are called on to give an opinion when the data that should guide you in that opinion are deficient. We want to know the previous history of this boy. If, instead of being in Hospital, this boy were a patient in private practice, and had been under your care, and you had been the attendant on his family for years together, and were familiar with him, and intimate with the state of his heart, you would be able to say, first, if he ever had carditis; next, whether, before his late attack, he had murmur in his heart or not; but we know nothing of all this; and the only fact we have to go on is the observation of Mr. Daly, that, when the boy was first examined, this murmur was not there at all. I myself have no doubt as to the correctness of this observation of Mr. Daly; my own opinion is, that, whether the murmur be organic or inorganic, it has been developed since the patient came into the house. Can we distinguish by acoustic signs alone,—and this is a point which bears on the subject of fever in a most important manner,—the inorganic from the organic murmur? The answer to that question is, simply,—that, in the present state of our knowledge, there are many cases in which we cannot do so; that there is no special acoustic character by which you can distinguish one of these phenomena from the other. This looks like a depreciating statement, as far as diagnosis is concerned; but the cause of diagnosis would be much more injured by attributing to it powers which it does not possess, than by confessing its deficiencies. The diagnosis, in the case in question, is to be drawn from other circumstances,—generally speaking, from circumstances connected with the condition of the patient, the absence of the signs of inflammation, and a variety of other points which we will consider more in detail on another occasion.

To come to the murmur in fever, the observation has been made in a considerable number of cases, that valvular murmur, when the patient is made to sit up, does not disappear; but, we have found in this Hospital that, in many cases in which a murmur was observed after fever, it was ascertained, that, when the patient was placed in an upright position, the abnormal sound disappeared, or, if it did not disappear altogether, it became greatly less intense; so that the disappearance of the murmur in the upright position is in favour of its inorganic nature, while its persistence or aggravation is in favour of its organic origin. So far so well. But, you will ask, is this rule absolute? That is a question which must be answered in the negative; for you will meet with cases of anæmic murmurs which are not influenced by position; and I believe there are, on the other hand, cases of organic murmurs which are influenced by position. There are, doubtless, some cases of organic murmurs in which, when the heart is made to act rapidly, the murmur either disappears, or becomes lost in the other cardiac sounds, so that you cannot distinguish it.

My own impression about the patient, whose case we are at present

studying, is that the murmur is inorganic. I trust it is; but I would not say so positively. I say this, because the character of the sound, although it is very aggravated,—although it approaches very closely indeed to the inorganic murmur,—is similar to a kind of murmur which I believe we, in this Hospital, were the first to describe, that is, the true muscular murmur of the heart,—a sound produced simply by the contraction of the muscular fibres when they do not contract *per saltum*, when they contract vermicularly, as it were; it possesses more this character than the character of the true valvular murmur. There is another point connected with it which is of importance. If this murmur was valvular, it would imply a great deal of disease; a rough, rasping murmur in the situation of the aortic valve implies generally a great amount of disease, and commonly of chronic disease; and, under these circumstances, you may be prepared to expect that the patient will show other signs of disease of the heart. So that we here have a diagnosis drawn, as I often observed to you before, from that most important source, the want of accordance of the symptoms. There is here, supposing the case to be organic, a want of accordance of the symptoms; for we should expect, that with this great amount of valvular disease, there would be signs of dilatation of the left ventricle, that there would be signs of dilatation; symptoms which commonly attend upon this form of disease of the heart. And yet here is a most curious fact, that even when this boy had one of his lungs almost entirely obstructed,—a condition which often acts in developing latent disease of the heart,—even at that time the symptoms of heart suffering were not at all remarkable. So that there is here, to a great degree, this want of accordance in the symptoms; and this is against the opinion of the disease being organic, and in favour of the view that the murmur is of a functional nature. The great mistake, that was made,—I am happy to say that it is now going out very fast,—in connection with auscultation generally, was this, it was supposed that every disease had its special acoustic sign, and consequently the attention of students and physicians was directed to the study of those signs in a purely mechanical point of view,—merely to the observation of their acoustic characters.

There can be no doubt that it is of the greatest possible importance to study carefully everything connected with a diseased organ, both its physical and its vital phenomena; but what you have to learn specially is this, not so much how to detect the sign, or how to recognise it as to know how to reason upon a particular sign when you have discovered it. It is here that the clinical student of long practice and experience has the greatest superiority over the mere reader. His mind is trained to reason upon the phenomena which he observes. Here we have a group of phenomena; and if we did not give ourselves the trouble of turning every possible point of the case over in our minds, we should come, I am sure, to a very imperfect and erroneous conclusion about it.

Bear this in mind always, that there is no pathognomic physical sign of any disease whatsoever. This cannot be too strongly stated; and I believe that we might go further, and say, that there is no combination of mere physical signs which, excluding the history and vital symptoms, can be justly considered to be pathognomic; at all events, if there be such a combination, it is one of extreme rarity indeed. We hear of certain murmurs being pathognomic signs of this and that disease of the heart,—of friction sounds being pathognomic of pleurisy—of crepitating râles being pathognomic of pneumonia—of amphoric sounds being pathognomic of effusion into the pleura. All this is wrong; it is based upon error; and you must expunge it altogether from your minds, if you wish to be accomplished physicians, investigators of truth, and faithful observers of disease as it is found at the bedside.

[The following remarks of Dr. Stokes on the connexion between typhus and tubercle, are exceedingly interesting. He says,]

I suggested that tubercular matter might be considered as one of the secondary deposits of typhus; and that we are justified in believing that, under certain circumstances, there may, with the same original or essential disease, be a variety in the nature of its secondary deposits. You will remember, that we examined two cases of tubercle in connexion with typhus; in one, the deposit occurred at the height of the fever, at the time when all the characteristic phenomena were best developed; in another, there was an interval of apyrexia between the subsidence of the typhus state and the appearance of the tubercular disease. Some might suppose, then, that there was no connexion between the second set of symptoms and the first; but is it not more probable, or, at least, is it not possible that the interval of apyrexia was analogous to the intermissions of an ague, and that the deposits in the second attack were still the result of the original disease; or, again, that the first attack had, as it were, prepared the entire organism for the elaboration of another form of deposit? I have before alluded to the fact, that, in the epidemic in which these accidents were most frequently observed, there was a strong tendency to relapse. And, from all these circumstances, I conclude, that the tubercular disease, in the second attack, may still be considered as the result of the typhus poison.

There are other points which this study of the connexion between typhus fever and tubercle helps to illustrate. One of the most important is, that we are led to look at tuberculosis under the aspect of an acute disease. Our views of it are, therefore, enlarged; for although what has been termed (though often improperly) acute phthisis has been described in books,—yet, in the minds of most men, the idea of chronicity is attached to tubercular disease. But we have seen, that a patient may die with typhus fever and its consequent tubercular deposit within fourteen days. And again,—we find, that

in the case of the consequent tubercle, that is where the disease has exploded after an apyrexial period, the patient may die with tuberculisation of all his organs, and without suppuration of the tubercle; or he may actually have innumerable softened tubercles in the lung and die of the disease within a fortnight after the commencement of the new attack. Such a patient may be said to have died from acute tubercle; but the term acute phthisis is improper, for the symptoms of consumption are by no means necessarily present.

In speaking of the case in which we made the diagnosis of the formation of tubercular deposits during the course of a petechial fever, I believe that I forgot to mention one remarkable symptom, namely, the extraordinary sweating to which the patient was liable. He had several paroxysms of sweating in the course of each day, and to such an extent, that when we removed the bedclothes clouds of vapour rose from the surface.

It is of importance that you should not allow erroneous ideas as to the constant chronicity of these affections to take possession of your minds. Even cancer, in some of its forms, may be a singularly rapid disease. I have observed large cancerous tumours to form under the integuments within a very few days before death. They were filled with the soft encephaloid matter. In this case, however, the patient had long laboured under a vast cancerous deposit within the chest, But I well remember another case which still better illustrates the occurrence of cancer as an acute disease. The patient was under the care of my friend the late Dr. Little, of Sligo. The case is an example of the rapidity with which organs may become cancerous, such as few persons who have not studied medicine at the bedside could have any idea of. It was that of a lad, aged about 18. He had long suffered from neglected ulcer of the leg. On admission into hospital, he had a vast corroding ulceration, engaging the whole leg from the knee downwards; and so extensive had been the destructive process, and so greatly had the system been exhausted by the suppuration and irritation of the enormous ulcer, that it was determined to remove the limb. For a certain time everything went on well, the wound healed kindly, and the patient not ouly recovered his appearance, but actually became fat. He was soon to be dismissed from the hospital, when he was attacked with pain of the right side, and symptoms of intense pulmonary irritation. The disease resisted all treatment, and he died within a fortnight from the period of the attack of the pain of the side.

On dissection, the lungs were found almost completely filled with masses of the soft white cancer. These masses varied in size from that of a pea to that of a large walnut. I presented the parts to the Pathological Society. Now. all this singular formation, or at least a very large portion of it, must have occurred in the short time that elapsed between the invasion of the pain and his death; for no one could suppose that with such an amount of disease there could have been perfect freedom from cough or difficulty of breathing.

Before we leave this subject, let me present it to you in another point of view. In these cases we see tubercular disease occurring as a general affection, and not merely as a disease of the lung. In these instances the affection of the lung was simply the production in that organ of a pathological state which engaged other viscera often equally and simultaneously. Now, although we do not say that there is no difference except as to rapidity and extent of deposit between these cases and those of ordinary phthisis, yet it is useful to bear them in mind in all our dealings with that fell disease. The progress of medical opinion is every day tending to the doctrine, that to consider consumption, as the word is usually understood, as an original disease of the lungs, is an error. Why it is that in so many cases the deposit appears to be first formed in the lungs, we do not know; nor why an attack of pulmonary irritation appears so often to be its exciting cause. Perhaps, as in the long, silent periods of an intermittent fever, the disease, even though it be a pure neurosis, still exists, there may be conditions in which the system is ready for the production of organic disease for an indefinite time. This may be true, not only with respect to tubercular disease, but also in many other essential affections, including a variety of fevers. And this disposition, this silent state of the malady, may itself pass away, and none may ever know how near the individual was to the invasion of a dangerous or fatal disease. I do not allude to what is called the latent periods of fever, but to a condition in which the occurrence of a fever or other essential disease is a possible or even probable, but not a necessary consequence. Be this as it may, of one thing we may be sure, that the less we consider phthisis as a pure disease of the lung, the better we shall treat it, and the less, day by day, will it be ranked among the opprobria of medicine. If consumption be not, strictly speaking, a disease of the lungs, but a morbid state affecting the whole economy, you can understand one cause, at least, for the too frequent failures of the attempts to cure it. If the pulmonary lesion be but the second step in the morbid process, or if not only the lung, but other organs are engaged,—what are you to expect from adopting the absurd, and often cruel, treatment so commonly used in cases of the disease? The blisterings, the leechings, the setons, issues, and eruptions by tartar emetic ointment, which still disgrace the practice of medicine, will be soon unknown among our more enlightened brethren. You cannot conceive the amount of suffering inflicted, and of positive mischief done, by the adoption of the doctrine that phthisis is to be treated as a localized, original, and irritative disease of the lung. Ask a practical physician as to his experience of phthisis, and he will often tell you, that, among the few cases in which a recovery took place, there were certain instances in which the patients had, either from despair or some other cause, pursued a course very different from that which is so often advised. I have known myself several most remarkable instances in which a temporary, or even a permanent, cure had taken

place, yet in which the patients had acted in every way contrary to rules. They had exposed themselves to all vicissitudes of weather; they had taken violent exercise, lived freely, and even drank hard. They recovered; and it is still a question whether this result was owing to the invigoration of the system by the return to their former habits, or to the escape from the enervating effects of warm rooms, relaxing climates, cough mixtures, sedatives, and slop diet: to say nothing of the barbarous issue or seton.

Let me give you an illustrative case. Some years ago I saw a gentleman, who came to town labouring under all the symptoms of well-marked phthisis. The disease had been of some months' standing, and the patient was a perfect picture of consumption. He had a rapid pulse, hectic sweating, purulent expectoration, and all the usual physical signs of tubercular deposit, and of a cavity under the right clavicle. I may also state, that the history of the disease was in accordance, in all particulars, with this opinion. I saw this patient in consultation with a gentleman of the highest station in the profession, and we both agreed that there was nothing to be done. This opinion was communicated to the patient's friends, and he was advised to return to the country. In about eighteen months afterwards a tall and healthy-looking man, weighing at least twelve stone, entered my study, with a very comical expression of countenance:—"You don't know me, doctor," he said. I apologised, pleading an inaptitude that belongs to me for recollecting faces. "I am," he said, "the person whom you and Dr. —— sent home to die last year. I am quite well, and I thought I would come and show myself to you." I examined him with great interest, and found every sign of disease had disappeared, except that there was a slight flattening under the clavicle. "Tell me," said I, "what you have been doing?" "Oh!" he replied, "I found out from the mistress what your opinion was, and I thought as I was to die I might as well enjoy myself while I lasted, and so I just went back to my old ways." "What was your system of living?" said I. "Nothing particular," he said, "I just took whatever was going." "Did you take wine?" "Not a drop," he replied; "but I had my glass of punch, as usual." "Did you ever take more than the one tumbler?" "Indeed, I often did." "How many? Three or four?" "Aye, and more than that,—I seldom went to bed under seven!" "What was your exercise?" "Shooting," he said, "every day that I could go out." "And what kind of shooting?" "Oh, I would not give you a farthing for any shooting but the one!" "What is that?" "Duck shooting." "But you must have often wetted your feet?" "I was not very particular about the *feet*," says he, "for I had to stand up to my hips in the Shannon for four and five hours of a winter's day, following the birds."

So this patient spent his day standing in the river, and went to bed after drinking seven tumblers of punch every night; and if ever a man recovered from phthisis he had done so when I saw him on that occa-

sion. Suppose, now, that he had been confined to an equable temperature, and a regulated diet, and had been treated in all respects *secundum artem*, what would have been the result? Any of you can answer the question. In point of fact, this very treatment had been adopted during the first three months of his illness, and his recovery may be fairly attributed not so much to the duck-shooting and whisky-punch, but to the general tonic and undepressing treatment which he adopted for himself, and which his system so much required to enable him to throw off the disease.

[Sometimes typhus, in its secondary form, attacks the windpipe, giving rise to what Rokitansky calls laryngo-typhus. It is in fact secondary typhus developed in the windpipe. Dr. Stokes says,]

In those cases which we have seen, the symptoms were, loss of voice, or a certain degree of hoarseness; the cough very rarely, indeed, had the laryngeal character, and I do not remember any instance of stridor in connexion with the other laryngeal symptoms. In some cases, the weakness or hoarseness of voice continued for a considerable time, and did not disappear until convalescence was far advanced. I have often thought, that, in these cases, the lesion of voice was to be attributed more to the weakness or paralysis of the laryngeal muscles than to any form of irritation or inflammation of the mucous surface. For, although we cannot bring any observations from dissection to throw light on this point, we may fairly believe that the laryngeal muscles are liable to be affected in typhus, just as the muscular fibres of the heart are found to be, and that the same process which causes a typhous deposit in the mucous surface may be repeated in the vocal muscles; and that a certain time must elapse before these organs recover their healthy condition. They may also be weakened quite independently of any structural change, just as we see to occur in the heart; for in this latter organ there are doubtless two forms of debility in connexion with typhus fever,—in the one we have weakness with distinct softening of structure; in the other, a debility which appears to be purely nervous. This leads me further to draw your attention to the probable existence of similar conditions in the circular, and perhaps, also, in the longitudinal fibres of the bronchial tubes in the secondary disease of typhus. I have little doubt that such a condition exists in many instances, and that a weakness, with or without softening of these structures, becomes an important element in the bronchial disease of fever. If, as some modern authorities have lately urged, these circular fibres are really the expectorating muscles, we can readily see how any weakness or paralysis affecting them would greatly increase the danger of a patient in fever, already suffering under copious secretion into the bronchial tubes. We can further understand, not only how this condition would superinduce what is termed effusion into the chest in fever, but also why it is that in the treatment of the bronchial disease of typhus there is such danger from

the employment of the antiphlogistic method, and how, on the other hand, such admirable results follow from the bold use of tonics and stimulants. I have before drawn your attention to the fact, that in many of our most remarkable examples of typhous softening of the heart, there was a great amount of secondary bronchial disease; and in such cases you will constantly see the associated diseases of the heart and the lung, progressing or retrograding simultaneously, the treatment adapted to the one being also adapted to the other. In the heart, so far as we know, we have only to deal with the affection of the muscular structure; in the lung we have at least two different forms of anatomical structure affected—the muscular and the mucous tissues. But the existence of the essential typhous state so far assimilates them that the vital condition of both is depressed in a somewhat similar manner; and it happens, that whatever is tonic and stimulant to the one is equally so to the other, and that to support and augment the vital energy of both structures, is the only means by which we can add the assistance of art to the efforts of nature in throwing off the disease.

[Sometimes typhus, in its secondary attacks, gives rise to *deafness.*]

Now, although I cannot give you any anatomical observations upon this point, yet I think I am justified in suggesting to you, that this symptom may proceed from a condition which is one of the secondary affections of fever, and that it is either a simple alteration of the power of the auditory muscles, or that this lesion is combined with structural softening, or, again, that it is a combination of a muscular with a mucous disease, both under the influence of the typhous state. Of one thing, at all events, we may be certain, that this symptom in typhus is not to be taken as a sign of inflammation either of the brain or its membrane, or of the ear itself. Like the other secondary affections of typhus, it supervenes in the course of the disease, spontaneously and silently. I have used this phrase before, and you understand what I mean by it. It usually comes on in persons who have had no predominance of nervous symptoms, in patients in whom we have had no occasion to direct attention to the head; it is rarely, if ever, preceded by, or accompanied by pain in the ear; it almost always affects both ears, though, in some cases, one organ is more affected than the other; it subsides without treatment, and I do not remember a case in which otorrhœa occurred as a symptom of the affection. You may meet cases of purulent discharges from the ears, especially in young persons of a strumous habit, who have been attacked with ear-ache during their imperfect convalescence; but such cases have no relation to the true typhous deafness with which we are now occupied. As I have said before, I have only clinical observation to bring to bear on this matter; we have no dissections to throw light upon it. One of the traditional prognostics in our fever hospitals is, that this deafness may be regarded as a favourable sign. However this may be, we have

rarely, if ever, seen a case in which it occurred prove fatal. A French author has lately published a memoir on the subject of deafness in connexion with the fever of Paris, and he refers to the results of several dissections, to show that it is a symptom of internal otitis. We can only say, that, whatever may be the case in the typhoid fever of the continent, such a view is quite inconsistent with our experience; and, even though we have no dissections to guide us, we may yet safely believe, that the deafness which occurs in the typhus of this country, is no proof of the existence of a local inflammation; and that in its rise, progress, and spontaneous subsidence, it is perfectly analogous to those varied affections which belong to the group of secondary effects of the one essential disease. We have, I repeat, only clinical observation to guide us here: but this is, in itself, a source of knowledge which cannot be too highly prized; one which, under favourable circumstances, gives us more trustworthy results than the most careful *post-mortem* examination. It is often more valuable to determine the state of the living organ, though we cannot see it, than to minutely scrutinize the dead parts. What would medicine be, if its great principles, maxims, and rules were founded solely upon what we see in the dead body.

You must not suppose that I undervalue pathological anatomy; but let me warn you against the error of considering it as a guide in medicine superior to the study of the living man, or as I might say, the living disease. The study of vital conditions is often found to excel *post-mortem* investigations in the evolution of useful results; and there is no class of cases in which this superiority comes out more clearly than in the essential diseases, and especially fevers; no matter whether we seek for a theory of the disease, or for rules of treatment. If we could infer the symptoms of fever from dissection of the bodies of its victims; if we could say that in this case there must have been one class of symptoms, and in another a different order and nature of symptoms; if we could, as it were, mentally re-construct the fever from its organic remains, then, indeed, what I have said would be an unsafe and hurtful doctrine. But it is not so; and the truth appears to be, whether we consider fever in its totality, or having reference to particular symptoms, that the worse fevers are those which produce the least organic change, and that when these changes are to be found, they are, as I have so often urged upon you, inconstant, unnecessary, and altogether incompetent to explain the nature or character of the disease.

Bear in mind, that medicine had attained a great value before pathological anatomy was cultivated. Its rise and progress were solely due to clinical observation; or, in other words, to the study of the vital conditions in disease, and the effects of remedies upon them. And admitting to the fullest extent the importance of the most minute study of the dead structures, especially when connected with the symptoms and history of disease, we must still, I believe, place

pathological anatomy in the second rank, as a means of extending our medical knowledge.

On the other hand, to hold that pathological anatomy has not advanced our knowledge of fever would be an error. If it has not told us what fever is, it has shown us what it is not; and this alone is a great boon. But it has done much more. It has taught us to know the singular though varying phenomena of the secondary diseases; it has shown them in their rise, progress, and spontaneous retrocession; it has taught us the re-action of new local organic disease upon them, and also their influence in modifying the laws of periodicity. It has not, however, enabled us to say when we find an organ free from change—that it was not functionally affected during life; nor, again, that where we do find structural alteration—that the disease may not have been latent, that is, unattended by any special or distinctive symptoms.——*Med. Times and Gazette, Feb.* 3, 1855, *p.* 103; *Dec.* 23, 1854, *p.* 633; *Nov.* 11, *pp.* 486, 485.

2.—ON FEVER.

By Dr. E. A. Parkes, Professor of Clinical Medicine in University College, and Physician to University College Hospital.

[One of the most eminent men in Europe, Professor Virchow, of Wurzburg, has defined fever as follows: "Fever consists essentially in elevation of temperature, which must arise from an increased tissue change, and have its immediate cause in alterations of the nervous system." Dr. Parkes at once admits the first part of this definition as we all must do. The second clause gives rise to a most interesting lecture from Dr. Parkes. He asks, what proof is there that the preternatural heat of fever must arise from an increased tissue change? We cannot prove this simply from the loss of weight of the body. How is the increased tissue change to be proved? We cannot collect two of the excretions—the cutaneous and the pulmonary—so as to measure them accurately.]

The other two excretions—the urine and the intestinal discharges—can be measured with accuracy, and it is fortunate that the urine in particular is a valuable measure of the metamorphosis of tissue. The urea alone represents two-thirds of the whole quantity of nitrogen which passes off, the sulphuric acid (the sulphates of the food being accounted for) represents almost entirely the oxidation of sulphur, and the oxidized phosphorus of the body passes out, in great measure, though not altogether, as urinary phosphoric acid. Therefore a careful examination of the urine, and of the intestinal discharges, with an approximative estimate of the pulmonary and cutaneous excretions, give us sufficiently extensive and accurate materials for the question at issue.

At present, opinions are widely divided, as to the conditions of the excretions in fevers. Some excellent observers hold that they are always, and of necessity, increased; others, no less exact, affirm that they are invariably, or almost always diminished.

If the pulmonary excretion—on which I have no personal observations—be excluded, I think I am in a position to prove that, in certain febrile cases, at any rate, some of the excretions are increased, in consequence merely of the febrile state, apart altogether from the specific disease which may be present; and I shall adduce, as briefly as possible, some examples of this:—The temperature of the body in these cases was judged of by measurement in the mouth or the axilla, and the normal heat of those parts is assumed to be 98° Fah. It is scarcely necessary to observe that, in speaking of the urine, I refer always to the amount of its solid constituents, and not the water.

Thus, a man, aged 26, had rheumatic fever. The temperature, for several days averaged at least 3° Fah. above the healthy standard. During these days the patient sweated profusely; he excreted on an average in each twenty-four hours 100 more grains of solid matters in the urine than in health, and this increase was particularly owing to augmentation of the urea, and the sulphuric acids. The intestinal excretion was as copious as usual. The food taken during this time was extremely small in amount, and the man was evidently and rapidly losing weight. Here, then, there was evidently a very great increase of tissue change, with coincident abnormal temperature.

In another case of rheumatic fever, the temperature was also for some days 3° Fah. over the standard; there was profuse sweating; the urinary solids averaged in each twenty-four hours 200 grains more than in the state of health, the urea and the sulphuric acid being particularly increased; the intestinal excreta were not apparently diminished.

In this instance, if we consider that no food was taken, and yet that the urinary ingredients derived from the daily metamorphosis of tissue were nearly one-fourth more than in the healthy state, when some 20 ounces of food were daily taken, the extraordinary disintegration going on in some tissue or other is evident.

But this is not peculiar to rheumatic fever.

In a case of typhoid fever, without increased intestinal excretion, the temperature was several degrees higher than natural; the skin was moist; the respirations were rapid, though there was none of the usual bronchitis, and the urinary solids were augmented in each twenty-four hours by about 60 grains, on an average, over the healthy amount.

Here, then, very little food was taken, the patient, as in the other cases, was quiet in bed; and yet the urinary and cutaneous excretions were greater than usual.

In a case of erysipelas of the head and face, with a temperature in the axilla of 1° over health, the skin was moist; the respirations were

quick (without bronchitis); the intestinal excreta were unaltered; the urinary solids (and among these the sulphuric acid, the uric acid in a less degree, and probably the urea) were augmented in each twenty-four hours very considerably over the healthy amount.

I have selected these cases, and those afterwards to be mentioned, because there appear to have been no modifying circumstances. No medicines were taken to complicate the result, nor were there any profuse discharges from the stomach and bowels, which are difficult to collect and estimate, and yet which reduce the amount of the urinary constituents.

Two at least of the excretions (the urinary and cutaneous) were then increased in these cases, while the other excretions were not apparently diminished. In some other cases, only one excretion is thus augmented:—

A man, aged 21, was admitted on the eleventh day of a disease, which was diagnosed as febricula. There was no local disease whatever, and there was no evidence of typhus, typhoid, or relapsing fever. On the eighteenth day, all symptoms had disappeared. Now, from the twelfth to the seventeenth day, inclusive, the average temperature of the mouth was 101·12° Fah.; and it fell gradually during this time from 102·25° to 99·5°. It averaged thus 3° over the normal amount. During this time, the bowels were confined; the respirations were not quickened (16—18 per minute), and the urinary solids were below the healthy standard. Thus, in each twenty-four hours during these six days, there were excreted by the kidneys only 476·416 grains; whereas the average of four days, when the man was perfectly well, was 567·455 grains in each twenty-four hours, or about 90 grains more, although at that time the temperature was only 97·9° Fah. Thus, neither in the pulmonary, the intestinal, or the urinary excretions, was there evidence of augmented chemical disintegration during the febrile period, but rather the reverse. But, then, the cutaneous excretion was evidently enormously augmented, for the sweating, day and night, was most profuse. Is it not reasonable to suppose that, in this case, it so happened that there was really increased disintegration of some tissue or other; but, that the products formed were of such a kind, as to be eliminated rather by the skin than by the bowels or the kidneys?

Again, in a case of typhoid fever, there was an average temperature of 101·29° Fah., from the fifteenth to the twenty-fourth days of disease inclusive. The average daily excretion of urinary solids during this time, amounted to only 362·971 grains; while six weeks afterwards, when the patient was well, and the temperature was normal, the urinary solids, examined during six days, averaged 436·001 grains. The urinary excretion was then diminished; but, then there was constant and considerable diarrhœa, and also, at one time, sweating and sudamina, so that the increase in these two excretions may be reasonably held to account for the urinary diminution.

In support of this proposition, that the excretions may be increased in fever, I may allude to some observations by Dr. Alfred Vogel; although, unfortunately, they refer only to the elimination of urea, and are unaccompanied by any statement of the temperature. Thus, in a case of typhoid fever, with diarrhœa, fatal on the sixteenth day; there were excreted on the thirteenth and fourteenth days respectively, 740 and 889·5 grains of urea—a very great amount, considering there was also diarrhœa. In another case, on the ninth and tenth days there were excreted 818 grains on each day; while in a third case, in twenty-four hours the excretion of urea reached the astonishing amount of 1065·5 grains,—or considerably more than twice the amount than would be secreted by a strong, healthy man, on exercise and abundant food. In this case, also, there were two liquid motions daily; no observation is made about the skin. The average amount of urea in all the cases of typhoid fever examined by Alfred Vogel, is of course not so great as this; but even including cases of convalescence, during which time the urea is much smaller, the average excretion in twelve cases (thirty-five observations being altogether made) was 560 grains, or considerably above any healthy average, except that given by Professor Bischoff, which is a little less than this, but is decidedly too high a standard for health.

In a case of pyæmia (a boy 19 years old), Vogel found in twenty-four hours no less than 1235 grains of urea, or nearly three times as much as in health. In a case of extended pneumonia he found, during the period of hepatization, and at a time when the respirations were 70—80 per minute, and the pulse about 100, the excretion of urea, on an average of two days, to be 695 grains, or more than 200 grains over the standard.

Now, as the urea is the chief representative of the nitrogenous tissues, it is impossible not to see, in the enormous amount excreted, a very rapid disintegration of these tissues; although, in Vogel's observations, the condition of the other urinary ingredients (except the chloride of sodium), and of the skin, is disregarded.

Without referring to additional observations, though there are several others,—among which I would specially mention those made by Heller,—I think I may be permitted to conclude, from the facts recorded in these six different febrile diseases, *that increase of temperature may be attended with increased elimination; and, therefore, presumably, with increased tissue change.*

But is this observation to be generalized? Does it hold good universally? Are the excretions increased in all cases of febrile heat? A most decided answer in the negative can be given. In many febrile cases there is diminution, instead of augmentation, of the excretions. I select the most striking examples of this which I have observed.

A man, aged 30, was affected with simple bronchitis of both lungs. The temperature averaged, during six days, 100·64° Fah.; the urinary solids, during the same time, amounted to only 294·174 grains in each

twenty-four hours, or not one-half of the healthy amount. Afterwards, during convalescence, when the body had not recovered altogether its proper tone, the temperature of the mouth averaged, during four days, 97·56°, and the urinary solids amounted to 406 grains in each twenty-four hours, or 112 grains more. Nor was the diminution of the urinary secretions in the febrile period compensated by increased elimination from the skin or bowels. There were indeed, some sudamina on the skin, but there was much less sweating than during the convalescent period, when the urinary solids were increased one-fourth. With respect the pulmonary excretion, it so happened that the respirations were not quickened, (twenty per minute), and we may presume that the rule laid down by Hervier and Saint Sayer, that the carbonic acid is decreased in pulmonary inflammations, held good also for this case.

To take another example—A man aged 48 was attacked with acute sthenic pneumonia, and came under treatment on the 9th day of the disease. The temperature for three days averaged 102·650; the urinary solids averaged during the same time only 369·17 grains, although, during convalescence, they rose to 589·45 grains; the skin was only slightly moist; the bowels were confined; the respirations averaged thirty per minute; the expectoration was scanty. Here, then, was a man whose temperature was nearly 5° over normal blood heat, and yet the urinary excretion was 200 grains less per diem than during the convalescent period, when it was still lower than in health. It is highly improbable, especially after the observations of Hervier, just referred to, that we are to look to the pulmonary excretion alone for the products of metamorphosis equal to 5° of heat.

I have other cases of pneumonia, showing the diminution of urinary ingredients, while the case quoted from Vogel, and others, which I could adduce from my own observation, prove that this diminution is neither constant, nor peculiar to pneumonia.

It so happens that these two cases are both pulmonary affections; but the same fact can be found in other cases.

This is an unequivocal case of typhoid fever in a man, aged 33, in which there happened to be no diarrhœa, no sweating, and no bronchitis. The urinary ingredients were for many days below the normal amount, though the elevation of temperature was considerable.

To take one more case:—A girl, aged 19, with acute rheumatism, and with a temperature averaging 102° Fah. in the mouth, passed for several days an extraordinary small amount of urinary solids; there was no compensation for this in other ways, for she was breathing tranquilly, the bowels were quiet, and there was only inconsiderable perspiration.

In all these cases, it is of course to be understood that there was no reason to suspect, either during the illness or after it, any condition of the excreting organs which would have impeded their functions had the substances they ordinarily excrete passed through their capillaries.

The conclusion is irresistible, *that the products of metamorphosis, as judged of by the excreta, may be diminished in febrile cases*, in which the heat of the blood is intense; and yet, in no inconsiderable number of cases of the same disease, with temperatures no higher, the products of tissue change, the excreta, are very greatly increased.

Since Nature knows no contradictions, how are these two classes of facts to be reconciled? Are the increased metamorphosis, and the increased heat, mere coincidences, and not really connected as cause and effect? All we know of the phenomena of normal heat forbids such a solution of the problem.

Other explanations are therefore called for, and of these there are several.

It is, in the first place, perfectly evident, that more chemical change may go on in the body than is represented in the excreta. The metamorphosis of blood, or of tissues, may not be carried to the point of forming those principles which can alone pass through the membranes of the eliminating organs. A vast amount of imperfectly oxidized compounds may be formed and retained in the system, circulating with the blood, or being thrown upon certain organs. We can, in fact, quite readily conceive increased metamorphosis with lessened elimination.

It is quite true, that direct analysis of the blood has not yet shown, in such cases, any actual retention and accumulation of the products of an imperfect oxidation; but there are some arguments in favour of such a view, which appear to me to have no mean weight.

Thus, it seems to me to be especially, though not solely, in these febrile cases with diminished excreta, that, at a later period of the disease, copious discharges from one or other of the eliminating organs suddenly occur. Thus, in the case of pneumonia, with lessened excretion, to which I have referred, severe spontaneous diarrhœa came on. In another case of pneumonia, with similar diminution in the excretions, violent purging and sweating subsequently came on. In other cases, diuresis has occurred, and an increase of urea, of sulphuric acid, and probably of uric acid, has been poured out.

It seems a reasonable explanation of these sudden discharges (usually termed critical from being coincident with more or less sudden fall of temperature and improvement in the other symptoms) to suppose that a large amount of partially metamorphosed substances have been retained in the body, and at length have been brought to that point of oxidation or change which permits their elimination by one or other organ. Then, by their discharge, the system is suddenly freed from the noxious compounds which weighed upon it; and the metamorphosis having reached its acme, the temperature immediately falls. Considered in this light, the so-called critical discharges, and the lowered heat, are mere coincidences.

I do not mean to assert, that it is only in these cases, in which diminished excretion is found during the early period, that the so-

called critical discharges occur. I believe they may then be generally expected; but, certainly, when the excreta are throughout abundant, such discharges may occur. We can suppose, then, that even the great amount of oxidation and elimination in such cases, does not suffice to get rid of all the material which has to undergo metamorphosis.

Another argument, which seems to me strongly in favour of the hypothesis of increased metamorphosis, with retention of the products, is this:—Secondary inflammatory affections appear to be more common in such cases, as if the blood were more contaminated. Sometimes, the connection between retention of excreta and local disease can be drawn closer; thus, sometimes in a patient, whose excreting organs are acting copiously, there occur a diminution of excretion, and a simultaneous and subsequent development of local disease.

Thus, in a case of rheumatic fever, the temperature of the mouth averaged, during three days, (fifth, sixth, and seventh of disease,) 100·37° Fah., the average excretion of urinary solids during the same time amounted to 1095·74 grains in each twenty-four hours, or at least 400 more than in health; and there was also profuse sweating. On the eighth day of the disease, the urinary solids suddenly fell to 493 grains in the twenty-four hours, and the sweating evidently lessened. There was no intestinal discharge to account for this. The temperature was only ·62 of a degree lower than before, and there was no alleviation in the general symptoms. Coincidently, however, with this lessened excretion, which affected all the ingredients in various degrees, a local inflammation (Angina faucium) became developed. On the following day, without change of treatment, the urinary solids augmented again, the angina lessened, and, in a day or two, disappeared. Does not the inference seem probable that, from some cause, there was, at a certain point, a sudden arrest of oxidation, and a consequent retention in the blood of hurtful substances which affected the nutrition and structure of a special tissue? It does not seem so reasonable to suppose that the angina faucium was the primary disease, and produced the diminution of excretion.

In a case of protracted typhoid fever, the urine in which was regularly analyzed during no less than fifty days, the average excretion of urinary solids, from the twenty-ninth to the thirty-seventh days inclusive, was 422·348 grains. Diarrhœa, which had been profuse, had ceased. There had been great sweating, but this had much lessened. In spite of the late period of the disease, the temperature during these eight days averaged 103·4° Fah. On the three last of these days, (the thirty-fifth, thirty-sixth, and thirty-seventh,) the urinary solids gradually diminished. On the thirty-eighth day, pleurisy came on, and lasted for about five days. During this time, the temperature was almost the same, viz., 103·44° Fah.; but the urinary solids averaged only 344·153 grains, viz., 78 grains less than during the previous eight days, though there was no increased sweating or diarrhœa to account for the decline.

On reference to the different urinary ingredients, the diminution was found to be most marked in the urea, the sulphuric, and the phosphoric acids, and to be less marked in the water and the chloride of sodium.

To take a third case:—A man, almost convalescent from a subacute attack of rheumatic fever, was passing a very considerable quantity of urine, which contained an excess of solids, and among these, of urea. The skin sweated moderately; the intestinal excreta were unchanged. Suddenly, without any very evident alteration in the cutaneous and the intestinal excretions, the urine decreased in amount both as far as the solid and fluid parts were concerned, to an extraordinary degree. Coincidently, there was a return of the joint affection, and on the next day there occurred a sharp attack of catching pain in the side, which may have been pleurisy, or perhaps pleurodynia, as friction sound could not be detected. Both the pleural or intercostal pain and joint affections rapidly disappeared, and coincidently, the amount of urinary excretion rose to its former standard.

These are the only cases in which I have been able to note the exact condition of the excretions in a febrile affection, at the very moment when a local disease appeared. I have not found any analogous fact in authors, but I think the precise nature of these observations forbids the supposition that the coincidence of the two phenomena was merely fortuitous.

The cause of this sudden arrest in excretion was not in any case apparent; but the effect was necessarily to cause disease of the blood by retaining in it substances which were really effete.

By the side of these cases, which show the retention of urinary ingredients, it is interesting to place the recent observation of Dr. Grohe, that in many effusions into the pleura and pericardium, in patients not labouring from Bright's disease, urea is discovered.

These various considerations appear to me to render the supposition most highly probable, that the diminished excreta in fever are to be referred to retention, but not to want of formation; that the excreta may be in fact small, though tissue change be great; and, if this be the case, Virchow's statement still holds good.

To account for the retention, many hypotheses present themselves: the fault may lie in a disproportion between the substances to be oxidized and the oxygen; a vast amount of albuminous substance, for example, may undergo partial change, and the oxygen, which can only reach to a certain amount, may be in insufficient quantity to convert all this changing substance into the principles of the excretions. Or the oxygen may be in some particular condition; for example, if it be true that in the body it is in an allotropic state, similar to or identical with ozone, and therefore in more powerfully oxidizing a condition, it may, in these cases, not have passed into that potent state, and therefore be incapable of causing so complete oxidation. Or, there may be a deficiency in the saline substances, without which oxidation is

checked: or, finally, there may be some special condition of the nervous system, which modifies transudation through the capillaries of the excretory organs. It is evidently impossible, at present, to discuss any of these suppositions, for their solutions lie, at present, beyond the powers of chemistry.

Although we can thus account satisfactorily, it appears to me, for the diminution of excreta in fever, it may be useful to see what explanations could be given of the preternatural heat, if the diminished excretions had sufficed to prove diminished tissue change.

It might be argued, in the first place, that the preternatural heat may still be referred to chemical change; but that this chemical action may be peculiar, and may lead, not to the formation of urea or sulphuric acid, but to that of some other compound, small in quantity, and perhaps unrecognized in analysis. Thus, we know that the combinations of some elements—of hydrogen, for example—take place, with the evolution of extraordinary heat; and yet the resulting compound may be small in bulk. It is impossible to say whether this hypothesis has any, or how much, truth. It is quite conceivably true, but it is at present supported by no facts.

Another explanation may however be advanced.

As in certain cases we have no evidence in the excretions of increased metamorphosis, and as the hypothesis of some unusual combinations of small bulk causing great heat is based on no facts, should we not abandon the strict chemical theory of animal heat? As in health some physiologists have looked for a second and recondite cause which produces some small degree of heat, unaccounted for, according to them, by chemical interchange, so in fever may it not be this particular cause which is especially perverted?

To this it seems sufficient to reply, that though the influence of the nervous system on chemical action, and through it, on animal heat, be fully admitted, physiologists seem unwilling to give a *locus standi* to the hypothesis which throws aside chemical action altogether. And as the diminution of excreta is susceptible of explanation in a way more accordant with the phenomena of other cases, and with physiological tenets, it is clearly unnecessary to entertain an hypothesis which has neither a physiological or pathological basis.

To recapitulate so far the results of the discussion:—Preternatural, like normal heat, arises from chemical action; which, in the former case, is augmented over the standard of the latter; this increased tissue change is evident from the loss of weight, and from the increase of one or other excretions; there are cases, however, and these are not uncommon, in which the excretions are diminished rather than increased; but this does not arise from lessened tissue change, but from retention of the products; the cause of the retention remains to be discovered, but principles, evidently effete, must circulate in the blood and organs; and it appears that it is in such cases that local diseases are most prone to occur; in such cases also, though not only in such,

great discharges from the skin, bowels, or kidneys occur, and constitute critical discharges. Finally, it would seem probable that the sudden check to excretion, which may occur in the course of febrile diseases, is attended with the same result as retention of the excretions from the first, and local inflammations occur.

[Dr. Parkes next considers some of the chemical points connected with fever. He continues:]

On looking over the materials which I have had the opportunity of collecting, one fact is very evidently indicated, viz.: that the degree of febrile heat cannot be measured by the amount of the excretions, as a whole, or by any special ingredient. That is to say, six patients, of pretty much the same weight and height, and labouring under the same or with different febrile diseases, and all having a temperature, we will say, of 102° Fah., will pass very different quantities of urea, uric acid, sulphuric acid, phosphoric acid, cutaneous and intestinal excretions. This is exactly what prevails in health, for, with an equal temperature, no two men pass exactly the same amount of excretory products. And, besides this, in fever the excretions, as we have seen, do not represent exactly the amount of metamorphosis, for even with increased excretion there may also be for a time a collection in the system of substances which are really effete.

The relative amounts of different excretory products, such as urea, uric acid, sulphuric acid, phosphoric acid, &c., vary so remarkably in the same disease in different persons, that it is difficult to know whether or not their varying quantity is connected with the febrile state alone, or with the specific nature of that state. This, perhaps, arises from the fact that we can never know the exact amount of urea, or sulphuric acid, which is *formed*, but only that which is *discharged*, and also, as already stated, from the circumstance that the febrile metamorphosis is still in some measure regulated by the law, whatever that may be, which determines the very different amount of tissue change in healthy individuals.

On looking at the nature of the excretory products of fever as far as these are known, it is at once evident, that they indicate chiefly a destruction of the albuminous or nitrogenized tissues. Thus it is yet doubtful whether the carbonic acid emitted from the lungs, the representative of destroyed fat and starch, is ever increased; but it is quite certain that the urea, the uric acid, the sulphuric acid, and the urinary pigment, the chief representatives of the azotized structures, are often very greatly so.

It would be interesting to determine, if it were possible, where the increased destruction of the albuminous structures occurs, whether in the blood, or in the organs. We know, most certainly, that, at the end of a febrile disease, the albumen, and the red corpuscles of the blood, are both lessened in amount. But this does not prove that the albumen has been destroyed at once in the blood; it may be that the

rapidly disintegrating organs have simply taken more of it, while it has not been recruited by the addition of food. The red corpuscles, however, do certainly undergo a more rapid destruction in the blood itself, or in organs, as the spleen or liver. Of the various tissues, none appear to waste so fast as the muscles, espscially the involuntary ones. The heart is well known to be often especially affected in some forms of fever, such as typhus; and of this a plausible explanation can, perhaps, be given.

Whether the liver, the spleen, the kidneys, and the lungs also waste, has not yet been determined; for several of the organs, and especially the spleen, are so affected by the specific disease, that it is difficult to know how far their structure is altered by the febrile affection, apart from the congestion or exudation which so frequently occurs in them.

Although the albuminous structures are thus especially destroyed, the fat of the body is also rapidly absorbed in fevers. It has not been shown, however, by accurate experiments, whether its disappearance is more rapid than would be the case in inanition alone.

I pass on now to a consideration of, perhaps, the most important general chemical condition of the febrile body; a condition which has been long partially observed, but has never been regarded as being of such interest as it really appears to be. I refer to the remarkable retention of water in the febrile system.

That the water of the urine is lessened in an extraordinary degree, sometimes even to one-half or one-quarter of its bulk, in spite of the vast quantities of fluid which are taken to quench the insatiable thirst, is a long-known fact. Professor Vogel, of Giessen, has lately directed particular attention to it, and has found the diminution to occur at the very commencement of pyrexia—an observation which I can confirm by many observations. It has been referred by some simply to the increased perspiration which was supposed to coexist, but accurate inquiry shows that this explanation is untenable. The urinary water is equally diminished when the skin is dry, and is often scantiest when the skin is driest. It has seemed to me, that in many cases the concentration of the urine is almost as good an index of the amount of fever as the temperature itself.

In all fevers, however, a time comes, when, though the heat is still preternatural, the amount of urinary water increases to its normal amount, or passes beyond it. In the specific fevers this occurs generally, either first, when the force of the cause has, so to speak, partially spent itself, or when the substances on which it acted have been expended, and when convalescence is approaching; or, secondly, when certain products of the metamorphosis are forming. In rheumatic fever, the increase of urinary water is seldom seen before the joint affection has almost disappeared; in typhoid fever it occurs earlier, in mild cases about the 14th to the 18th or 20th day, although the febrile heat is still one or two degrees over the standard. If

there be great thirst, as is often still the case in typhoid fever at this time, the amount of urine is sometimes excessive ; its specific gravity is low, and its absolute acidity is less. In a case of relapsing fever I found the urine in small amount during the febrile period, and abundant at the period of remission, when the skin also was very moist. In pneumonia the quantity of urine seldom becomes large till the softening is far advanced, although sweating occurs earlier.

If we remember the large amount of fluid which is drunk in fever, the retention of the water will appear still more remarkable. But there is another order of facts equally interesting.

Great attention has been directed during the last few years to the amount of fluid which is poured into the alimentary canal from the various glands and membranes. It is now known that, in varying degrees, there is a constant transit of fluid from the blood into the alimentary canal, and as rapid reabsorption. The amount thus poured out and absorbed in twenty-four hours is almost incredible, and of itself constitutes a secondary or intermediate circulation never dreamt of by Harvey. The amount of gastric juice alone, passing into the stomach in a day, and then reabsorbed, amounted in the case lately examined by Grunewald, to nearly twenty-three imperial pints. If we put it at twelve pints we shall certainly be within the mark. The pancreas, according to Kroeger, furnishes twelve and a half pints in twenty-four hours, while the salivary glands pour out at least three pints in the same time. The amount of the bile is probably over two pints. The amount given out by the intestinal mucous membrane cannot be guessed at, but must be enormous. Altogether, the amount of fluid effused into the alimentary canal in twenty-four hours amounts to much more than the whole amount of blood in the body in other words, every portion of the blood may, and possibly does pass, several times into the alimentary canal in twenty-four hours. The effect of this continual outpouring is supposed to be to aid metamorphosis ; the same substance, more or less changed, seems to be thrown out and reabsorbed until it be adapted for the repair of tissue or become effete. Some medicines thus pass from, and repass to, the blood many times in determinate parts of the canal before a combination occurs which facilitates their exit from the frame. The exact steps of this wonderful extravascular circulation are at present unknown, but its extent obliges us to consider it as one of the greatest importance.

Now, in fever, this intermediate circulation undergoes a great diminution, if not, in some cases, an almost total arrest. Thus, very early in fever the buccal mucous membrane becomes sickly, and the amount of saliva diminishes. So constant is this, that an old writer includes dryness of the mouth in his definition of fever. We well know, that a very early symptom of improvement is the moist tongue, caused by the return of salivary flow, and by the increased quantity of mucus. The decrease in the quantity of gastric juice is quite as

certain. Independent of the fact that loss of appetite is constant in fever, we learn, from the experiments of Dr. Beaumont, that, when St. Martin was feverish, no flow of gastric juice could be excited in any way, although at other times even the mere introduction of the thermometer into the stomach caused copious secretion. The amount of pancreatic fluid in fever is quite unknown; but, as little food is taken, it cannot be so great as in health, and, judging from analogy, it is diminished like the saliva. The succus entericus, like the gastric juice, is probably diminished, for the stimulus of food is taken away; and we also know, that, in febrile affections (cases in which there is specific affection of the mucous membrane being excluded), there is constipation, and the fæces are dry. The resort to purgative medicines is, therefore, almost invariable.

We are, therefore, entitled to conclude that, as in the case of the urine and the sweat, the intermediate circulation in the alimentary canal is more or less diminished.

The consequence of such an abnormal state of things are yet obscure. If the object of this extravascular circulation be to prevent abnormal, and to aid normal metamorphosis, as supposed by Schmidt, the most serious impediment to this must, of course, arise. And here we have one of the many anomalies of fever—we have, as coincident phenomena, increased tissue change, and lessened amount of that circulation which is supposed to aid metamorphosis.

It is easy to conceive, also, how the nutrition of the mucous membrane of the alimentary tract must suffer, when that transit, with which, of course, their anatomical structure is in unison, is thus impaired.

Are we to stop here, and suppose that the lessened transudation of water is confined to the skin, kidneys, and alimentary mucous membrane? or are we to suppose that the extravascular circulation is still more profoundly altered? In health, we know, that in every liquid, in every organ in the body, there is, from molecule to molecule, from cell to intercellular fluid, from fluid to cell, a continual and rapid movement of fluid, arising, not simply from transudation or endosmose, but by the agency of a chemical action on the surface of membranes, which repels and (attracts?) with an almost inconceivable power.

Without venturing to pass the cautious limit which the great chemist who has discovered the osmotic force and its laws, has prescribed, we may yet well suppose that the chemical changes and the development of heat are intimately connected with this molecular movement. Then at once comes the question, whether this vast circulation, to which the circulation discovered by Harvey is but the servant, is altered in fever? We can scarcely avoid concluding that it is so, when we observe so many evidences of altered metamorphosis, of altered heat, and of altered transudation, in those

parts which are accessible to observation. And how can metamorphosis and animal heat be altered, without the process with which they are connected being also changed? But at present it is impossible to do more than presume, that as physics advance, and our means of research become more adequate to the task, we may be enabled to pass into these penetralia, whither, at present, no clue leads.

The retention of water in the system, to which I have referred as the most general chemical fact connected with fever, is at present incapable of explanation. Like the augmented metamorphosis of fever it seems to be connected with some unusual condition of the nervous system, and even, in so-called health, it has been shown by the elaborate researches of Beneke, that when the nervous system is depressed, and a semifebrile state is produced, the flow of water by the kidneys is at once diminished. But at present it is impossible to determine the immediate physical cause of the retention. Is it to be looked for in some chemical condition of the vessels or the membranes of the excretory organs? If so, every excreting and secreting organ must alike suffer, and this is in accordance with facts. But then most certainly different organs suffer unequally, a phenomenon which can only be met by the further hypothesis, that different organs are unequally affected?

Another explanation may be suggested. It is now well known that during endosmosis different organic substances absorb or take very different quantities of water through membranes. For example, the endosmotic equivalent of sugar is a large amount of water, and in consequence of this physical law, sugar attracts water from the blood, and carries it from the system. Hence the unquenchable thirst of diabetes; for every structure is robbed of its water by the powerful attraction of the diabetic sugar. Is it possible that in fever some substance is produced which has as powerful an attraction for water as sugar, and the action of which may cause the universal thirst?

A fact mentioned to me by Mr. Graham, will put my meaning in a clearer light. Mr. Graham has discovered that gelatine has an extraordinary attraction for water, so that it will even take it from alcohol, and render alcohol almost, if not quite, anhydrous. This property, manifested at all temperatures, is particularly marked at the temperature of 98° to 100° Fah.

Albumen, on the other hand, has little attraction for water, and yields it up at once to alcohol.

Now supposing that, in the rapid metamorphosis of albuminous substances in fever, gelatinous compounds, or something approaching to them, were formed,—and this is by no means unlikely,—then, as a consequence of a physical law, the gelatine would at once take water from the albuminous tissues and would necessarily give rise to intense thirst. Then, unlike sugar, the gelatinous substance

would not be discharged, but must be converted into urea and uric acid, as ordinary gelatine is when it is taken as food.

I mention this hypothesis merely as an example of how water might be retained in the febrile body. At present, of course we do not know whether any such compound is or is not formed, for the transition steps, and they may be numerous, between organized albumen and urea, are not yet known.

At any rate, whatever be its antecedent phenomena, the retention of water in the febrile system is one of the most remarkable of its chemical phenomena.

There seems some reason to think that besides water, chloride of sodium is to a certain extent, also retained in fever, or, at any rate, that it passes off less readily than usual with the urine. I say some reason, only, because this point cannot be said to be at present properly worked out. In all fevers, the amount of chloride of sodium in the urine rapidly falls. This is owing partly to the withdrawal of food; but it cannot be altogether and always so owing, because I have found the chlorides absent almost, or altogether, in several febrile diseases, in some cases of typhoid fever, of rheumatic fever, of erysipelas, of bronchitis, and have seen them return again when the fever lessened, before food was again given. In these cases the chlorides did not pass off by the intestines, or the lungs, and in the special cases of typhoid fever and erysipelas there was no sweating, so that they did not escape from the skin.

In the paper by Alfred Vogel, to which I have formerly referred, the chlorides were found to be always diminished. This is ascribed, in the history of the cases, to deprivation of food; but when Vogel sums up his conclusions, he seems almost disposed to take a different view, and says that, allowing for the food, the chlorides seem to be rather diminished than increased.

Passing now from these general statements, I ought to consider in proper order, the chemistry of the excretions, of the secretions, of the blood, and of the organs.

But it would be a mere pretence of knowledge, were I to assert that anything satisfactory is known of the chemistry of the secretions or of organs. I pass on then to a consideration of the excretions, and the blood.

I. *The excretions.*—The general characters of the urine proper to the febrile state have been already incidentally referred to, and need be merely recapitulated; they are, deficiency of water, increase of solids, if there be no retention; among the solids, the urea, uric, hippuric, sulphuric and phosphoric acids, and the pigments are increased; the condition of the oxalic acid is uncertain; the chloride of sodium is diminished; the condition of the creatine and creatinite is not known; unusual products, as leucin or albumen, or modifications of it may be present, but are not essential.

There are only two additional points to which it is necessary to refer.

The deep colour of febrile urine has usually been attributed to its concentration, but it is now certain that this is only a partial explanation; if febrile urine be diluted to the usual amount of healthy urine, it is still darker than usual. Professor Vogel has lately shown that the colouring matter is increased sometimes fourfold. This dark pigment contains apparently more carbon than usual; some portion of it rapidly takes oxygen when exposed to the air, and forms lactic acid, and when the urates fall, which they soon do, in consequence of the greater concentration and acidity of the urine, as well as, in some cases, from their own greater amount, this colouring matter falls with, and variously paints them. When these deposits are washed with alcohol, they yield some portion of the colouring matter to it, and this does not give any of the reactions of bile pigment. Indigo can, I believe, be formed from it, as it is from common pigment, according to Dr. Hassall's interesting discovery.

The importance of the pigment consists in the fact, that it may apparently be considered as a measure of the metamorphosis of the blood globules, and from its amount, we are led to conclude that, in some cases of fever, the disintregation of the blood particles may be four times as rapid as in health.

The other fact connected with the urine is the augmentation of the free acidity as measured by neutralization with soda. Unfortunately, at present, it is not absolutely known whether the excess of acidity is present when the urine is passed, or whether it is caused by subsequent acid fermentation of the pigment. But, considering the increase in the uric, hippuric, sulphuric, and phosphoric acids, it probable that it is in excess before any changes occur in the urine from exposure. The causes of the increase, remain, of course, to be investigated when the fact is certain.

II. The exact chemical constitution of the cutaneous, and of the intestinal excretions, are yet unknown. In certain specific fevers, indeed, both excretions have been partially studied; but to this subject I shall, of course, not refer.

The pulmonary excretion has been very imperfectly examined. In all exquisite inflammations, except those of the lungs and heart, Hervier and St. Sager found the carbonic acid augmented. Lehmann, however, criticises these observations, and, apparently on good grounds, denies their accuracy. In typhus, it seems certain, from the observations of Malcolm, that the carbonic acid is very greatly diminished. In a case of pleurisy, lately examined by Gorup-Besanez, it appeared to be increased.

III. *The blood.*—The composition of the febrile blood has attracted the greatest attention and interest; but, owing to the extreme difficulty of separating its constituents, its chemistry, in febrile affections, as in almost all other diseases, has yet really to be investigated. The observations of Andral and Gavare, Simon, Becquerel and Rodier, Denis, and others, valuable as they are in many respects,

belong really to the very infancy of science, and are utterly incapable of solving the mysteries of a fluid like the blood, in which are found the most complex combinations of the most complex principles, and in which circulate materials both for nutrition and for elimination. The blood is, indeed, like the sea, which feeds the clouds and receives back the rivers laden with the impurities of the earth. The venous system especially, from the blood of which analyses are generally made, is the great sewer of the body, into which all organs drain their disintegrating effete structures. How can the analyses of such a fluid represent the composition of the blood at large; and how is it possible, in the present state of chemistry, to attempt such an analysis with any success?

There are, however, some facts of interest. It would certainly appear from the observations of Cahen, who has, we are told, made more that 300 experiments, that the alkalinity of the serum of the blood is diminished considerably, by about one-third, in inflammations. And, by the side of this statement we may place the analyses of Becquerel and Rodier, which show in inflammation a diminution of the alkaline salts.

Cahen states that the lessened alkalinity does not occur in typhoid fever, and it has even been asserted that the alkaline salts, and, therefore, presumably, the alkalinity, are increased in this disease. Cahen appears to found his statement, that the alkalinity is not decreased in typhoid, on a single case; I have a single case to put in opposition to this, in which by neutralization with acid the alkalinity of the serum was found to be decidedly lessened. The statement that the alkaline salts are increased in typhoid is quite erroneous, they are slightly decreased.

Whatever be the case in typhoid fever the alkalinity of the serum is lessened in rheumatic fever during life; and Professor Vogel has noticed also acidity of the serum after death, in a case of rheumatic fever, in which death occurred from another cause. From observations after death the serum appears to be sometimes even acid in pyæmia and puerperal fever. In these cases, however, the acidity may have been produced by post-mortem changes.

The diminution of the alkalinity runs parallel with the chemical fact, that the alkaline salts and the chloride of sodium are diminished in the venous blood in inflammation, in rheumatic fever, in typhus fever, in tpyhoid fever, and, perhaps, in typhus.

Another established fact connected with the serum of the febrile affections is the diminution of the albumen after the fever has lasted some time, and the commensurate increase in the water of the serum. Whether the albumen is altered in quality or in composition is not known. The observations of Frerichs on the presence of leucin have been already sufficiently mentioned.

The augmentation of the fibrine in many diseases is one of the few well-ascertained facts in the chemistry of the blood. It is now well

known not to be peculiar to, though most constant in, inflammations; it will occur in the specific fevers as in relapsing fevers; it is a constant condition of the blood in rheumatic fever, and I had an opportunity of showing, some years ago, that it may precede for several days the affection of the joints or heart, although Andral and Gavarret vainly sought after such a case. It is evidently, however, not necessary to the febrile condition, and therefore, need not be discussed here.

The alterations of the red particles in febrile diseases are almost totally unknown. They certainly diminish in numbers during the course of the disease, but whether their individual constitution suffers change is not yet known. In certain specific fevers, the malarious for example, they may be destroyed without being eliminated, and form pigment cells and masses, as lately pointed out by Meekel and Planer.

It may appear disheartening to confess, that the results have been so incommensurate with the labour bestowed on the analysis of the blood. But on this subject the chemist's first words have scarcely yet been spoken, and we must wait almost for his last, before his conclusions are available for pathology. We must remember the intricate nature of the problem, and not too impatiently blame the inevitable tardiness of science. But a few years since, we should have thought it impossible to detect the wonderful difference in the salts of the serum and the red particles, or to count the myriad multitudes of these latter structures. Now, these marvels are almost among the simplicities of science. But a few years since the detection of uric acid in blood was looked upon as chimerical, and now, thanks to the ingenious process of an esteemed colleague, the merest tyro can do what the profoundest chemist formerly thought impossible.

We may be sure that chemistry has hidden powers yet to be revealed which will be competent to solve even this most difficult of tasks. Till it is so solved, however, the chemistry of the blood in pyrexia must remain what it is now, a chaos of uncertainty and contradictory assertions.—*Med. Times and Gazette, April* 7, 1855, *p.* 333.

3.—TREATMENT OF YELLOW FEVER BY TURPENTINE.

By James Laird, Esq., Surgeon, R.N.

From various documents which have been forwarded to us, we gather the following interesting facts respecting the remarkably successful treatment adopted during a recent outbreak of yellow fever on H.M.S. Medea. The disease broke out in the beginning of August, whilst cruising on the South Coast of Cuba, and continued to rage for upwards of a fortnight, during which a sixth part of the ship's company suffered from it. During this epidemic, and a subsequent relapse, about sixty cases came under Mr. Laird's care, and of these but four

died. The principal treatment pursued consisted in the exhibition of turpentine in small doses, as first recommended by Dr. Gilmore King. The supposed modus operandi of the remedy was by elimination, in virtue of its action on the skin and kidneys. The dose usually ordered was fifteen minims, with a little nitric ether and camphor mixture given every three or four hours, and continued until a remission was apparent. As soon as the remission occurred, quinine was resorted to, together with a more nutritious diet. As adjuvants, castor-oil, occasional doses of blue pill, evaporating lotions, sinapisms, cupping, blistering, &c., were employed. Regarding the further details of the treatment, &c., the following extracts of a letter from Mr. Laird will be the best explanation:—

"With regard to bloodletting, I have to state that this remedial agent was had recourse to in a few of the cases, chiefly because the practice appeared to be of service during the Bermuda epidemic of 1843.

"On the important question, however, of bloodletting in yellow fever, I have been led from my late experience considerably to change my opinion, and although I took blood from the arm only to a moderate extent in nine instances out of more than sixty cases of yellow fever treated on board, I am now convinced, limited as my practice in this respect was, that venesection in this disease, however early and moderately practised, is a remedy extremely hazardous, and one which might, except in some particular cases, be safely dispensed with.

"Local bleeding, by means of cupping over the epigastrium or nape of neck, as found requisite, will, I now believe, generally answer the purpose required.

"In respect to the therapeutic effects of turpentine in yellow fever, particularly its almost certain action on the skin and kidneys, I cannot speak too highly.

"Except in cases attended with much irritability of stomach, the draught I gave did not appear to cause nausea, and that annoying symptom, strangury, only occurred in six cases. In the last eleven cases put on the sick list, after leaving Port Royal for Halifax, I gave the turpentine in doses of ten instead of fifteen drops with equally good effect, and without the occurrence of strangury in a single instance of this number.

"In no case did this remedy appear to act as a stimulant by exciting the pulse, an objection which I have heard was made to its use by some of the medical officers who served during the Bermuda epidemic of last year; but this objection I believe to be erroneous, from repeated observations I made on the pulse fifteen and twenty minutes after the administration of the turpentine draught.

"When applied externally, as was done in the case of a sailor-lad with black vomit and suppression of urine, who would not take the draught,—turpentine mixed with equal parts of olive oil, and rubbed

in over the body and limbs, seemed to act even better than if it had been taken by the mouth. Three hours after the frictions were applied, this patient made a pint of healthy-looking urine, and, after an interval of eight hours, he passed a pint and a half more urine of the same character, at one micturition. Early next morning, black vomit, which had been present for about thirty-six hours, entirely ceased, and the lad appeared otherwise much improved. The frictions were continued at intervals, but no medicine whatever was given by the mouth. He made urine freely; the pulse had become stronger, and he appeared as if quite certain of recovery, when, after struggling violently to resist the administration of a nutritive enema, to which was added ℥xxv. of tincture of opium, to secure its being retained, he was soon after seized with symptoms of congestion of the brain, under which he ultimately sank.

"On dissection no trace of black vomit could be detected, unless a little dark green matter found in the ileum might be considered as such. The mucous lining of the stomach was much congested, and discoloured dark red; but its epithelium was perfectly intact, and although the liver presented the usual fawn colour, the skin was not tinged yellow, as was the case with the other patients who died with black vomit. This case, I might be excused saying, ought to have been a recovery from black vomit, had not the unlooked-for head affection come on, with which I cannot but regret to think the opium might have had something to do, notwithstanding the small quantity administered.

"In another case of black vomit, I gave turpentine in doses of 40 drops, combined with medium doses of hydrocyanic acid, with the evident effect of controlling this fatal symptom, although the patient was carried off soon afterwards, by profuse and uncontrollable hemorrhage of fluid-looking blood from the mouth and nose."—*Med. Times and Gazette, April* 14, 1855, *p.* 358.

4.—ON SOME POINTS IN THE TREATMENT OF GOUT AND RHEUMATISM.

By Dr. Fuller.

[The following is the abstract of a paper read before the Western Medical Society of London.]

Dr. Fuller commenced by stating, that in all diseases, but especially in those which, like gout, rheumatism, and rheumatic gout, are attributable to faulty assimilation, and the consequent formation of a morbid matter in the blood, the success of treatment must necessarily depend upon the care and judgment bestowed on four several points:—first, the history of the case; secondly, the appearance and general condition of the patient; thirdly, the character of the excretions; and fourthly, the selection and adaptation of proper remedies to meet the

various exigencies of the case. He then proceeded to point out the bearing of each of these considerations upon the course of treatment to be pursued; and after drawing attention to the close relationship subsisting between the amount and character of the various excretions, and the condition of the system generally, he insisted on the necessity of providing for the full and perfect action of *all* the excretory organs. He instanced cases which had fallen under his care at St. George's Hospital, in which the long continuance of the disease was evidently referable to the sluggishness of some one particular organ; and others, in which the obstinacy of the symptoms was attributable to the imperfect action of *all* the excretory organs, rather than to extreme sluggishness in *any one*. These latter cases, when not attended by fever, and when characterized by affection of the muscular structures, are often benefited by bark and hydrochlorate of ammonia, which, under such circumstances, have the effect of promoting an increased flow of all excretions, and so of eliminating the materies morbi, and checking the disease. Dr. Fuller, however, dwelt strongly on the fact, that very different remedies are needed to regulate the various excretions under different conditions of the general health. He showed that when the system is low and deficient in vital energy, tonics, such as bark, quina, sarsaparilla, steel, and cod-liver oil, together with change of air, change of scene, freedom from anxiety, and the use of proper baths, and a due amount of exercise—form the most effectual means of checking the malassimilation on which the formation of the poison depends, and of increasing the action of the excretory organs, whereby its elimination is brought about, and that unless such remedies are exhibited, and the tone of the system improved before colchicum, guaiacum, and other so-called "specifics" are administered, no good results can be produced. He attributed the failure of treatment, in many instances, to want of care in tracing out the history of the case, whereby it happens that purely neuralgic pains, as well as the pains which follow the incautious use of mercury, and the pains and swellings resulting from a gonorrhœal taint are confounded with gout and rheumatism, and are wrongly treated accordingly. He regards the too frequent habit of treating disease in accordance with its name rather than according to the condition of the patient, and the character of the secretions, as another cause of failure, inasmuch as it leads to the practice of prescribing each of the so-called specifics separately and apart from other remedies. He pointed out, that in most cases the action of *all* the excretory organs is disordered, and that whereas no single remedy will excite them all equally to action, the judicious practitioner should employ such a combination of remedies as will prove effectual for the purpose.

Dr. Fuller combatted the opinion advanced by Dr. Garrod as to the non-existence of such a disorder as rheumatic gout, and avowed his belief that the three forms of disease which form the subject of this paper, are referable to different forms of malassimilation, and the con-

sequent formation of different poisons in the blood; and he urged the remarkable differences observable in the age, sex, and physical condition of the sufferers from rheumatic gout, in the ascertained causes of the disease, the mode in which it makes its attack, its symptoms, its behaviour under treatment, and the pathological changes to which it gives rise, as evidence in favour of its being a disease essentially distinct both from gout and rheumatism. He admitted the value of Dr. Garrod's uric acid test as a means of distinguishing, in many instances, between gout and rheumatism, as well as between true and rheumatic gout; but he did not admit the absence of uric acid from the blood as proof of the disease being true rheumatism. Moreover, admitting the value of the test theoretically, he denied its practical utility. Its only indicational value is in regard to the administration of colchicum, and even in this respect it cannot be trusted. In no cases is lithic acid more abundant in the blood than in those old cachectic cases of gout, accompanied by the formation of chalk-stones in the joints, and by granular degeneration of the kidneys; yet in none is colchicum of less avail, or more ill borne by the system. On the other hand, when the blood is free from lithic acid, as it usually is in true rheumatism, colchicum is often of great service, by acting as an evacuant, and modifying the secretions of the kidneys, liver, and bowels. Colchicum, therefore, should not be relied upon in all cases of gout, neither should it be discarded in all cases of rheumatism; and in the one, as in the other, the history of the case, the condition of the patient, the stage of the disease, and the character of the excretions, must be our guides to the administration of that as of other remedies.—*Lancet, Feb.* 3, 1855, p. 127.

Dr. Fuller, in his recent work upon rheumatism, regards the pathology of the disease as depending upon the presence of a "*materies morbi*," which he supposes to be an excess of lactic acid, owing to defective assimilation, or suppressed perspiration. With the view to the neutralisation and elimination of this acid, he recommends the free use of alkalies, by which means he "hopes the average duration of an attack may be reduced, from a month or six weeks, to ten days or a fortnight."

Dr. John B. Chapin states (*New York Medical Times*, August 1854), that, during the attendance of Dr. Swett, at the New York Hospital, the present year, all the patients admitted with acute articular rheumatism were put under the "alkaline treatment;" and he gives a statistical table of twenty-five cases treated by this method.

The salt chosen was the tartrate of soda and potassa, a neutral salt, possessing the property of rendering the urine alkaline.

"The plan of treatment usually pursued was, if the patient presented himself with unusual excitement of the skin and pulse, to administer a mixture of sulphate of magnesia and tartarised antimony,

until the skin was relaxed, and the pulse reduced to a more natural standard. The Rochelle salt was then directed, in drachm doses, every two or three hours during the day-time, till the urine was rendered alkaline, when it was gradually suspended. A lotion of carb. potass. ℥j., with opium ℥ij. to the pint of water, was directed as an external application. The administration of the salt was not attended with disagreeable consequences, with the exception occasionally of some ulceration about the fauces; in no case was its action so severe upon the bowels as to require its entire suspension. The persons attacked were in the full vigour of health, and the character of the disease acute in its form. The frequency of administration of the remedy was governed very much by the re-action of the urine.

"On the admission of the patient, the urine was tested, and in all cases was found to be of acid re-action, and the secretion of the skin presented the usual acid odour. The treatment was generally commenced the second or third day after admission, and the urine was rendered of decided alkaline re-action in an average of five days after its commencement; the longest period it resisted the alkaline re-action having been twenty days, and the shortest two. The secretions of the skin have not, I believe, been noticed to alter. In one case, attended with profuse perspiration, which yielded readily to treatment, the coloured shirt the patient wore entirely lost its colour; and it was suggested whether the same change did not take place in the perspiration as in the urine. The average amount of the salt administered was from five to seven ounces.

"The average date of commencing improvement was seven days after commencement of treatment, coinciding, in the large majority of the cases, with the commencing alkalinity of the urine. The improvement was invariably permanent, and, after the urine was rendered alkaline, no new articulations were affected, as a general rule.

"The average period of convalescence was twelve days after admission; and the whole duration of the disease, including the period previous to admission, was twenty-two days. Of thirty cases treated by Dr. Swett, during April and May, 1850, during which time no uniform course of treatment was pursued, the average duration was five and a-half weeks. One of the most gratifying results of the alkaline treatment was the diminished frequency of cardiac complications. Twenty-one of the twenty-five were free from any complication; three were admitted with aortic obstruction, and one with mitral regurgitation. Not one patient was attacked with any heart complication during the treatment of the disease. Comparing this result with the practice last year, it was found that four had mitral regurgitation, six aortic complication, and three suffered from pericarditis; thirteen in all, out of thirty."—*Med. Times and Gazette, Dec.* 16, 1854, p. 621.

5.—REMARKS ON GOUT.

By Dr. A. B. Garrod, Physician to University College Hospital, &c.

[Dr. Garrod has arrived at the following conclusions on this subject, after making upwards of 200 examinations of the blood for the purpose:]

1. That in all cases of genuine acute gout—that is, in all patients where the great toe is specially affected, and where the other symptoms are such as to leave no possible doubt as to the diagnosis—uric acid exists in the blood in abnormal quantities.

2. That in all cases of acute rheumatism where the symptoms are characteristically marked, with or without heart affection, no abnormal amount of uric acid is present.

3. That in all acute or chronic forms of the same diseases—even when not so characteristically marked at the time of the examination—where the maladies can be distinctly traced to true gout or rheumatism, the same difference exists in the composition of the blood.

4. That the effused fluid, such as the serum, obtained from a blister applied to a non-influenced part, contains uric acid when the blood is abnormally loaded with this body.

The method for the most part made use of, and which is found to yield very trustworthy results, is very simple in its nature, and capable of being clinically applied, has been named the "Uric Acid Thread Experiment," consisting in the immersion of a single fibre or two of unwashed huckaback linen into a drachm or so of the serum of the blood, previously acidulated with a few drops of acetic acid, put into a flat glass dish, and placed aside, in order to allow spontaneous evaporation, when, if an abnormal amount of uric acid be present, the fibres will be covered with the crystals of this body. The blister serum may be treated in the same manner, using only a rather larger quantity—not less than two fluid drachms. Another mode of diagnosis found very valuable, is the searching for small deposits of urate of soda, or miniature chalk stones or tophi, which are frequently formed on the cartilages of the ears; sometimes, but less frequently, about the integuments around the eyes; sometimes on the palmar surface of the tips of the fingers. These concretions may, and do very often, exist in these parts, but especially in the ears, without being present at any other part; and as no deposit of urate of soda has ever been found in any patient without the blood containing an abnormal amount, and no case of genuine rheumatism was ever accompanied or followed by their formation, their presence may be regarded as an indisputable or pathognomonic sign of a gouty diathesis. Dr. Garrod detailed a case affording many points of interest. Gouty patients are frequently extremely susceptible to the influence of mercury, and, in the above case, a few grains of blue pill. Another point of great interest is, that this case shows clearly that even during the first four days of inflammatory action, urate of soda may be effused into the tissues, and indi-

cates the importance of cutting short the inflammation, if possible, to prevent the mischief which may otherwise take place in the joints. This, however, must be done by relieving the system, and not by suppressing the local affection, or worse results will probably occur. In another case, there was, and had been in previous attacks, absence of the great toe affection; at least, the patient did not remember that that part had ever been specially inflamed. Here was also the absence of any deposits in the ears or elsewhere, and it was solely from the examination of the blood that the author could assert that it was acute gout rather than acute rheumatism. However, in subsequent attacks, of which she has had two during the last two years, he has been enabled fully to test the accuracy of my first diagnosis. The blood has uniformly given an abnormal amount of uric acid crystals. The blister serum has contained the same.—*Med. Times and Gazette, Jan.* 13, 1855, *p.* 44.

Dr. Gairdner "cannot look on the disappearance of urea and uric acid in the urine, and their accumulation in the blood, as anything else than a very frequent symptom and consequence of gout, itself again being the cause of other important phenomena." It is venous congestion which he regards as the first condition essential to the formation of the gouty diathesis. We may select the following passage as presenting a summary of his views:—

"This hyperæmic condition of the viscera, and loaded state of the blood-vessels, is very unfavourable to secretion. The functions of the body are imperfectly performed under such oppression. The consequences are a diminished flow of bile and loaded bowels; suppression of the matters usually evacuated by the kidneys; the disappearance of the unctuous moisture which bedews the interstices of the fingers, toes, and arm-pits; and a rapid addition to the general plethora. The heart now feels the unusual load, and by palpitation and struggle seeks to rid itself of the incumbrance; the veins become more turgid, the foot grows tender, the valves lose their office, and the capillaries must sustain unaided the whole weight of the superincumbent column of blood. If this state be not checked, a paroxysm of the disease soon occurs. This termination is sometimes promoted by standing on the affected limb, sometimes by the heat of the bed during sleep. The pain of the fit continues unabated till the swelling and œdema begin; in fact, till the strained and distended vessels have relieved themselves by the extravasation of their fluids. Patients even observe that the relief of the pain is coetaneous with the appearance of œdema, and the return of moisture to the skin; the swelling, so long as it is dry, hard, and elastic, affording no relief. At the same time, the solid matters of the urine begin to reappear in that secretion, and the bowels, which have hitherto required the strongest cathartics to move them, are now readily opened by the gentlest aperients, the flow of bile, in fact, is re-established. This, I believe, is the true *ratio signorum* of a fit of the gout.—*Monthly Journ. of Med., Dec.* 1854, *p.* 537.

6.—*Gout treated with Piper Methysticum.* By Dr. E. W. Pritchard, Hunmanby, Scarbro'.—[Dr. Pritchard's patient had tried the usual remedies for a severe attack of gout but found no relief.]

Seeing it was a well-marked case of podagra regularis, and that my patient, though corpulent, showed signs of much debility from previous treatment, and moreover was exceedingly irritable, I ordered the limbs to be enveloped in piline, saturated with the tincture of piper methysticum, enjoined perfect tranquillity and freedom from business matters, and to take the tincture of piper methysticum every four hours in the following form:—

℞. Tinct. pip. meth. ℥ss.; aquæ puræ ℥vj. Capiat quart. part. quaquâ horâ.

To drink lemonade, and to have for his dinner manna croup puddings; for his tea coccoa-nibs, boiled in milk, one half-pint.

On the 4th, found he had slept uninterruptedly since taking the fourth dose of his medicine. Did not see him till the evening, when he was feverish and anxious, chiefly on account of resting so long, declaring I had over-dosed him with opium. After a good deal of persuasion, I induced him to continue the treatment.

5th. He ate a hearty breakfast of fried bacon and toast, with boiled milk, and expressed himself much better; that he enjoyed his breakfast like he did months ago, before he was afflicted with this distressing malady. Constipation troubled him a good deal. I ordered the following pill:—℞. Pil. rhei. comp. gr. iv., cap. j., nocte que mane, and that he was not to mind his bowels acting at present, for the action of the piper is more marked when retained in the system. This was merely to calm him, for as a principle I do not give aperients in gout.

7th. He expressed himself another man, and from this time he went on improving, and continued under treatment about a fortnight; recovery was complete, and yesterday I received a letter from him, saying, I ought to publish the case, as he was quite well, and, though 65 years of age, as active and in good spirits as ever he expected to be.—*Med. Times and Gazette, Dec.* 9, 1854, *p.* 591.

7.—*Treatment of Acute Rheumatism.*—[Many of the London physicians now seem to treat rheumatism chiefly by the neutral salts, either by the acetate, tartrate, or nitrate of potash. Lemon-juice is still held in esteem by some, but by others it is used as an adjuvant without relying on it alone.]

Dr. Barlow, of Guy's Hospital, is accustomed to state to his clinique, that the most rapid recovery from acute rheumatism that he ever witnessed was under treatment by the acetate of potash. We may quote the following case as a fair example of the usual treatment pursued by that excellent practical physician, and also of its average results. The plan of treatment is, however, by no means peculiar to

Dr. Barlow. H. T., a strong, robust man, aged 33, was admitted on the third day of his first attack of acute rheumatism. The disease was severe. Ordered to take a draught containing half a drachm of acetate of potash, ten grains of the nitrate of potash, and ten minims of the vinum opii, diluted with barley-water. For nine days he was kept on low diet, and on the tenth the improvement was so far advanced, that the decoction of bark was substituted for the barley-water in the prescription, and a better diet was allowed. On the fourteenth day he was convalescent, about the ward, and marked for discharge in a few days.—*Med. Times and Gazette, March* 3, 1855, *p.* 213.

8.—*Acute Rheumatism treated by Bicarbonate of Potash.* [Dr. Garrod, within the last three years, has had to treat, in University College Hospital, 51 cases of this disease.]

The main part of his plan of treatment consists in the administration, in a diluted form, of two-scruple doses of bicarbonate of potash, every two hours, day and night, until the patient has been free from all articular affection and febrile disturbance for two or three days, using local depletion over the heart's region, if any cardiac disease is present or threatened. The author detailed three cases of rheumatic fever, illustrating this mode of treatment: the first, a girl, ten years old, in which the duration under treatment was five days, the the total duration eight; the second, a young man, aged twenty, with a complication of heart disease, where the duration under treatment was eight, the total duration fifteen days; the third, a young woman, aged eighteen years, in the fifth attack, the former ones having always lasted for a month or five weeks, but which, by the adoption of his plan, yielded in nine days, total duration being but thirteen days, four having elapsed before her admission into the hospital. He afterwards gave a table of fifty-one cases of acute rheumatism; and of each patient the following particulars are noted:—The age, occupation, hereditary predisposition; the number and causes of attack; the symptoms before admission; the symptoms during treatment; the nature of treatment; and the duration of the disease. From these cases, the following deductions are made—viz., that in twenty males the duration of the disease under treatment averaged between six and seven days, and the total duration between eleven and twelve days; and in thirty-one females, the disease under treatment averaged from seven to eight days, and the total duration between fifteen and sixteen days—giving in all an average under treatment of seven days and a half; and, for the total duration, about thirteen days and a half. The author then alluded to the influence of the bicarbonate of potash when administered in large and frequent doses upon the different organs and functions of the body; and remarked, that it produces neither nausea, vomiting, nor purging—in fact, no symptom of gastro-intestinal irrita-

tion. It now induces a strongly alkaline condition of the urine, causes it to effervesce freely, with excess of acid, but does not appear to promote an increase in the quantity of the secretion. It appears to render the secretion of the skin less acid—sometimes almost neutral. That it acts as a powerful controller of the heart's action, reducing greatly the frequency of the pulse, but without causing the faintness often produced by digitalis, colchicum, &c. That it probably increases the alkalinity of the serum of the blood, and diminishes the coagulability of the altered fibrine occurring in rheumatic fever; and hence, probably, checking or preventing the deposits of lymph on the endo- or peri-cardium. He (Dr. Garrod) stated his opinion, that the influence of the bicarbonate was felt not only in shortening the duration of the articular affection, but also in preventing or moderating the cardiac disease. After enumerating many details of the method adopted, and the value of certain adjuncts, as opium, calomel, and occasional general depletion, he proceeded to recommend a plan of treatment which, from his experience, he considered calculated to ensure the greatest amount of success, and thought it probable that the total duration of the disease might, on the average, be reduced to about ten days,—provided that the treatment was adopted early, and no serious complication existed.

Dr. Copland inquired whether the author of the paper had used the bicarbonate of soda, and what were the results. The alkaline treatment of rheumatism had been long in use. In his treatise on that disease he had recommended, after the proper evacuation of the bowels, that the decoction of bark with alkalies should be resorted to. He mentioned some cases in which the disease appeared to have been cut short by the following plan of treatment:—A powder, consisting of half a drachm of magnesia and the same quantity of milk of sulphur was given night and morning, and bark with alkalies, as mentioned above, three times during the day. Alkalies had been recommended many years since by a friend of his, in a work entitled, "The Simple Treatment of Disease."—*Lancet, March* 3, 1855, p. 239.

9.—DISCUSSION ON CANCER.

By M.M. Velpeau, Robert, and Delafond.

[Considerable difference of opinion exists in Paris and London on the use which may be derived from the microscope in the diagnosis of cancer. M. Velpeau does not place confidence in it. The following are M. Robert's remarks on the same subject. He says,]

You are aware that, according to the microscopists, the cancer-cell is the specific characteristic of cancer. I will not pause to discuss, as M. Velpeau has done, the relative value of the cells, the nuclei, and the nucleoli; for, although I have certain convictions of my own on the subject, I think it is better to leave questions like this to those men who have studied them *specially*, and not to meddle with topics

about which we know little practically. You will perhaps say that I am adopting the definition of cancer given by the microscopists, totally forgetting that my opponent refuses to accept their opinions. M. Velpeau has said that the cancer-cell cannot be the specific anatomical character of cancer, because it has been found in non-cancerous tumours. I would ask my learned colleague by what authority he declares the tumours which contained the cells in question, not to have been cancerous? Because, he will reply, they did not return after extirpation. From this it would follow that every tumour which does not recur after removal, although it contains myriads of so-called cancer-cells, is not cancer; and from this I should also be correct in concluding that the so-called cases recorded in M. Velpeau's work, at p. 585, were not cancers at all. And, by carrying this mode of reasoning still further, I could prove that my colleague has not, any more than Munro and Macfarlan, cured a single case of cancer.

The physiological are not less marked than the anatomical characters of cancer. The disease has a constant tendency to become generalized, and manifested elsewhere, either in the neighbouring lymphatics, in some other organ, or in the viscera themselves, especially in the lungs and the liver. Before cancerous tumours ulcerate, and injure the constitution by profuse suppuration and hemorrhages, we often see the signs of general cachexia become manifested. The malady constantly returns after extirpation, and the progress of cancer becomes much more rapidly fatal in exact proportion to the frequency of its recurrence, and the operations for its removal. I have often noticed this fact, but was never more struck by it than in the case of a woman, aged 38, who, after four successive ablations of a small tumour of the mamma, had three recurrences of the malady in less than two years, and who returned to the hospital three months after the fourth operation, where she died from the recurrence of a multitude of similar tumours within the chest.

Fibro-plastic tumours are always circumscribed, and have not the irregular ramified *(rameuse)* form which characterises certain cancers, especially scirrhus. On their first appearance they possess a firm, slightly elastic consistence; they are homogenous in structure, and contain no juice. Their tissue is termed by Lebert, *fibro-plastic*, and it is composed of cells, and of fibres which exactly resemble the elements of cellular tissue during their development. These tumours advance but slowly, and they are, for a long time, quite compatible with all the appearances of good health. Nevertheless they may become softened, ulcerated, and even generalised, and they may occasion death by being developed in the viscera. Of these facts M. Velpeau has given us several examples. But even among those rare cases in which the malignity of the fibro-plastic tissue rivals that of true cancer, we find consoling facts, which M. Velpeau ought not to have passed over in silence. Of these facts, which are now very numerous, I shall mention a few.

Paget (in his 'Lectures on Tumours,' 1853) mentions a case quoted from Maclagan, of a girl, who, in 1832, was operated on for a firm, elastic, fibrous-looking tumour, in the left lumbar region. It returned after a year, and was again removed in 1834. A second recurrence happened after a period of eighteen months, and the third operation was completely successful; for in 1850—fifteen years after it—she was, and had continued until then, in perfect health.

In 1839 I removed from the right axilla of a young man, a large tumour, the size of both fists, which was circumscribed, lobulated, but firm and elastic, and appeared to be composed of fibrous tissue intermixed with fat. In five years thereafter it returned, and I operated again in October, 1844, removing a tumour, the size of my fist, possessing the very same appearance as the first. Again it returned after the lapse of other four years, and for the third time it was extirpated, in January, 1849. M. Lebert examined the tumour, and pronounced it to be fibro-plastic tissue mixed with fat. Since this time the patient has enjoyed perfect health.

Again, Mr. Syme, in 1839, removed a tumour, situated over the first rib, from a man aged 48. A second operation was necessary in 1843, a third tumour was removed in 1847, and a fourth in 1849, and a fifth in 1851. Professor Bennett examined these tumours microscopically, and found them to be composed of fibro-plastic tissue. Shortly after this fifth operation another tumour, of a considerable size, was formed under the clavicle. During all this time the patient's general health was never impaired. Ultimately this tumour ulcerated; hemorrhage took place; and, worn out by it, the patient died in July 1852. *There was not a trace of the malady to be found in any of the internal organs.*

In September 1849, M. Chassaignac removed a tumour from beneath the left clavicle. In a few months it reappeared, and a second operation was performed in December 1850, a third in July 1851, a fourth in January 1852, a fifth in July the same year, and, finally, a sixth in October 1853. Since that date, M. Chassaignac has not seen the patient. During the course of these operations the general health continued excellent, and there was no implication of any of the lymphatics. The tumours were examined each time by M. Lebert, who pronounced their structure to be purely fibro-plastic, mixed with a little gelatinous matter.

When we reflect on the course of all these fibro-plastic tumours, on the obstinacy of their recurrence, and on the fatal termination of many of them, we certainly cannot regard them as of a benign nature. But, on the other hand, are we correct if we class them with cancerous tumours? Has an encephaloid or a scirrhous tumour ever been extirpated for four, five, or six recurrences, without the disease becoming generalised, and the constitution cachectic? And can the instances of fibro-plastic recurrence in the viscera be compared, for frequency of

manifestation, to the more constant return of cancer in the glands, and within the cavities of the chest and abdomen?

I would next speak of *epithelial affections*, which are composed anatomically of epithelium cells or of the epidermis which normally covers the skin and mucous membranes. I cannot conceive how M. Velpeau can say that the structure of such tumours (if we are to believe the microscope) renders them akin to benign growths, such as warts, corns, &c. What does it signify, that two tumours have in common a normal anatomical element, if this element be not disposed and combined in the same manner in each? In warts and corns, the epidermis, though more abundant, stands in exactly the same relations to the other parts of the skin as it does in the normal condition of the parts. We shall find on examination that this is not the case in an epithelial tumour. M. Velpeau's assimilation then, is more specious than real.

Epithelial tumours appear principally on tegumentary surfaces, and ought to be studied separately according as they are situated on the skin or the mucous membranes. On the skin they begin by small crusts (*boutons crouteux*), warts, or fissures, and they form ulcers called *cancroid*, on account of their differing in appearance from cancer, progressing more slowly, and having less tendency to infect the system. The utmost evil that they do is to affect the neighbouring glands. When treated by energetic appliances, calculated rapidly to destroy the indurated parts, they often become healed definitely and radically. The epithelial tumours of the mucous surfaces (as types of which I shall specify those of the tongue and cervix uteri) are different in their progress; their course is more rapid; they reappear almost invariably after extirpation, and they terminate by causing death. Their tendency to generalization, however, is very far inferior to that of true cancer. If we examine the bodies of those women who have died of these deep ulcers of the cervix, we may often find the uterus partly destroyed; and the bladder, peritoneum, and great blood-vessels may be perforated by the disease. But, in such cases, we never find epithelial masses deposited in the viscera; the utmost traces of the disease which we can discover are the slightly enlarged glands in the neighbourhood of the uterus. It is the same with epithelial ulcers of the tongue; the glands of the neck alone become affected; and when the patients die, it is rather from being exhausted by hemorrhages, and from the long continued deglutition of putrid matters, than from any generalization of the disease having occurred.

At the orifices of the body, where the skin and mucous membrane become blended together, epithelial tumours possess, what I call, *mixed* characters, which they communicate to the complexed structure of the tissues affected. But there, as on the skin, the tumours continue for a long while in the condition of local maladies; and although it may be proved that their extirpation is less frequently curative than of those on the skin, it can also be demonstrated that the radical cures

effected by the operation are very highly encouraging. I am quite aware that epithelial tumours *may* become propagated or reproduced in the lymphatic glands, and even in the viscera; but such instances of generalization are very rare.

These, then, are the differences, both of the minute structure and the mode of progress, of the three lesions which are generally confounded together under the term—cancer. We cannot consider epithelial and fibro-plastic tumours as benign in their nature, and we certainly cannot, in point of malignity, rank them with cancerous maladies properly so called; M. Velpeau does not admit these distinctions, for to him all these lesions are cancers, and he does not recognize the speciality of any of their elements. If the patient *must* sink, "what does it matter," he asks, "in *what manner* he dies,—whether by the disorganization of the diseased parts or from the results of general infection?" I would answer, in the first place, that all these maladies are not alike in their progress and severity; but even supposing that they *were* all equally fatal, is it not important to study them, in order to establish the differences between them, and to endeavour, sooner or later, to apply the knowledge thus gained to their scientific treatment? I feel convinced, that at present, the only question concerning cancer that is at all settled, is that of prognosis. Is it nothing for us to know that although the removal of a cancer leaves the patient exposed to the danger of an almost certain recurrence, sooner or later followed by death,—that the extirpation of an epithelial or fibro-plastic tumour is much more hopeful and satisfactory in its results? If, in the latter case, a return of the disease should take place, the surgeon may operate again with greater hope and courage, knowing well that a radical cure is not impossible.

Let us, then, study these diseases more and more, and let us endeavour to distinguish them from those maladies with which they are often confounded. Let us strive to learn still more accurately their clinical differences; and, in doubtful cases, let us always avail ourselves of the aid afforded us by the microscope.

M. Robert expressed his dissent from what had been stated by M. Delafond, that the cancer-cell was not characteristic or specific in its appearance and shape. M. Delafond had asserted that the form of this cell was not constant in its form; that it was spherical or ovoid when it was not compressed by the surrounding tissues, and that it was elongated or fusiform when it existed under opposite conditions. These plausible statements were not borne out by facts, for he had often seen ovoid and spherical cells in the densest scirrhus, as well as elongated cells in softened encephaloma; and moreover, fusiform bodies were common in subcutaneous fibro-plastic tumours, which were quite exempt from compression. M. Delafond had stated that the volume of the cells varied according to the phase of their development, but this was quite unproven. M. Robert exhibited a series of drawings illustrative of the appearance of the cancerous,

fibro-plastic, and epithelial elements, and also of pus globules, and he showed that, although occasionally they might present somewhat abnormal aspects, there could be no difficulty, in the great majority of instances, in recognising them, and distinguishing them from one another.

Another objection raised against the specific nature of the cancer-cell was, that in several of the normal tissues of the body, we found cells which presented a very great analogy, if not a perfect similitude, to those of cancer. This view originated with Virchow, and for a time was popular in Germany. According to that pathologist, the pavement epithelium of the mucous membrane of the ureters, the bladder, the lungs, and the conjunctivæ, resembled very much the cells of cancer; and scarcely a year ago M. Michel, of Strasbourg, had traced the same analogy in the cells of the medulla of the bones of young children. M. Robert had examined all these structures, assisted by able microscopists, in order to ascertain the truth of the statements; and after repeatedly examining and comparing together the tissues mentioned above and the elements of cancerous growths, he never experienced any difficulty in distinguishing the different structures. It might be asked, Why have distinguished histologists in other countries arrived at different results? Might it not be due to differences in the power of the microscopes used? A microscope of high powers revealed structural appearances which were invisible to any one using an instrument of lower powers. Cancerous elements appeared very differently when viewed under powers of 300 and 500 diameters. He thought, therefore, that the objections raised against the specific character of the cancer element were more specious than real, and he maintained that an experienced eye could always distinguish them from other structures, whether normal or pathological.

M. Velpeau had principally contested the specific nature of the cancer elements in a pathological and clinical point of view; and he alleged that he had found cancer-cells in non-cancerous tumours, which were extirpated without the occurrence of any relapse. But M. Robert held that, if M. Velpeau admitted the curability of true cancer by operation, the non-recurrence of the tumours in question did not prove them to have been non-malignant. M. Velpeau said he did not found his diagnosis on the non-recurrence of tumours, but on the general appearance of the patient. But among all the admitted clinical signs of cancer, there was not a single pathognomonic unmistakeable symptom, when the cachexia was absent, and generalization of the disease was not yet manifested. Modern histology was daily showing us that the symptoms reputed to be characteristic of cancer could not be relied upon as such, since they were being constantly met with in morbid productions of a totally different nature. For example, it had been held, from time immemorial, that retraction of the nipple, occurring in tumours of the breast, was a pathognomonic sign of scirrhus. But M. Robin had lately described a form of mam-

mary hypertrophy, in which this retraction of the nipple was very well marked, and which consisted simply in a hypertrophy of the glandular *culs de sac*, and in atrophy of the excretory ducts, which give to the breast the aspect of scirrhus. Two cases of this, which had recently been witnessed within a very short time, led him to believe that this affection was by no means rare.

Again, when a mammary tumour was extensively ulcerated, when its edges were fungous, sprouting (*vegetans*), and its base was greyish, sanious, discharging fœtid pus in great abundance; and when this was accompanied by hemorrhage and exhaustion, we said that we had to deal with a serious case of cancer. But, to M. Robert's own knowledge, a combination of all these alarming symptoms had occurred in two cases of simple hypertrophic tumours. In one of these cases he himself had operated with complete success, and the patient had for five years enjoyed perfect health. The other case was similarly successful in the hands of a celebrated surgeon in Paris.

But M. Velpeau had further asserted that the specific cancerous elements had been found absent in truly cancerous tumours. He had searched M. Velpeau's book in vain to find proofs of this.

M. Velpeau had also stated that tumours which at first contained no cancer cells, had been known to recur, having these cells in the secondary tumours; and he had cited several cases of this nature; but in all these cases the *primary* tumours had not been microscopically examined, and therefore there was no proof of their non-cancerous nature.

As to the benignity or malignity of the tumours, he thought that it was better to direct our attention to their prognosis and manner of evolution than to their anatomical structure. *All tumours were relatively benign or malignant.* A true scirrhus, which had existed eight or ten years, without implicating the glands, or causing pain or cachexia, and which, after its removal, did not recur, or did so only after the lapse of a few years, was extremely benign when compared to an encephaloma, which ran through all its phases, and killed the patient in less than a year. The same was true of epithelial, fibro-plastic, and other tumours not so well known, which recurred after ablation. A *noli me tangere* of the cheek, or ala nasi, was a hundred times more benign than an epithelioma of the under lip, of the rectum, or of the cervix uteri. And farther, such a scirrhus as he had described was much more malignant than a simple adenoid tumour, of large size, which was ulcerated, and discharging fetid pus profusely. If, therefore, in the majority of cases, structural characters were constant and easily demonstrated, nothing could be more variable and uncertain than the prognosis and diagnosis we might base simply upon the evolution of morbid products.

We had then two problems to solve; that of structure, and that of prognosis. The solutions of these were not antagonistic, and we ought not to seek only to solve one of the questions. But the science

of the present must precede that of the future, and anatomical diagnosis must precede prognosis.

This view appeared to him a specific and a practical one. What had been done respectively by the anatomical and clinical men, at the bedsides of the patients? The former had divided tumours into cancerous, epithelial, fibro-plastic, vascular, glandular, &c. By long and patient study they had become acquainted with the delicate characters of the tumours, which were long passed by unobserved; and they had tried to discover in what particular lesions particular elements were found, and thus they had frequently been successful in establishing a correct diagnosis. They knew that one tumour might remain for a long time unremoved, without serious injury to the system; that another could not heal spontaneously; and that the former was more benign than the latter, &c. They thus formed a mental estimate of the patients' history, weighed their good and bad chances, and finally were able to say in what cases it was right to operate, and in what other cases it was prudent to leave the disease undisturbed.

But what had the purely clinical men done? Was their judgment, based on traditions of doubtful value and on pure empiricism, better than the more complete and exact views of the anatomical school? When one of them was called to treat a tumour, if he found that the patient had a bad aspect, and sinister physiognomy, he removed the growth, like an anatomist. But while the latter, in such circumstances, strove first to confirm, by means of the scalpel and the microscope, his first impressions as to the nature of the disease, in order to strengthen his practice if he was in the right, and to modify his first opinions if he had been deceived, the former, limiting the circle of his researches, made an uncertain diagnosis and a doubtful prognosis.

It was more philosophical and conformable to science to base our classifications of tumours on their anatomical composition and structure. If the microscope had dispelled many illusions as to the curability of true cancer, it had also revealed to us the possibility of curing diseases which often, but for it, were allowed to proceed to a fatal termination. The microscope could not, any more than pathological anatomy in general, decide emphatically in every case, the benignity or malignity of a morbid production, but it enabled us, better than mere clinical observation, to forsee the result of treatment. In certain cases it gave us caution, and in others boldness. By means of the microscope we had made great advances in the study of all accidental growths; and if we became hereafter perfectly acquainted with the obscure question under consideration, much of the praise would be due to those who had brought so much ardour and laudable perseverance to these truly difficult researches.

M. Delafond discussed at a great length the various cell-theories, and quoted the opinions of a great many authors in support of the doctrine of cellular unity. Besides the authority of the German micro-

scopists, Vichow, Schwann, Förster, Müller, he cited passages from the works of MM. Lebert and Broca themselves, which were favourable to his opinion. He terminated his address by enunciating the following conclusions, which he thought were proven:—

1st. Independently of *normal* cells, there exist in the organism *pathological* cells, which cannot be distinguished by fixed and constant characters.

2nd. In the origin of their formation, the three cancer-cells—(the cancer-cell properly so-called, the epithelial, and the fibro-plastic cells), present no marked differences among one another.

3rd. If, under such circumstances, these cells, especially fibro-plastic cells, present differences of form and dimensions, these differences are only due to the varying densities of the tissues in which they are developed.

4th. The chemical characters, by means of which it has been endeavoured to distinguish cancer-cells, are not sufficiently well-marked to constitute a trustworthy distinction.

5th. The effect of chemical reagents are equally inadequate to afford a sufficient ground of distinction.

6th. Numerous facts have proved that true cancers do not always contain the cells called cancer-cells, while these cells have been found in tumours and tissues which were not cancerous.

7th. Still more numerous facts prove that epithelial tumours, which are not considered to be true cancers, are capable of recurrence not only at the same place, but at a distance from it.

M. Velpeau.—I have just published a work in which I affirm that cancer is sometimes curable, and that it can be very often diagnosed without the aid of the microscope. M. Robert having disputed these propositions. I have tried to defend them. *Firstly*, I have endeavoured to prove that cancer can be diagnosed independently of the aid of the microscope; but I have not meant by this to say that the microscope was useless, and that the works of microscopists were of no value, as has been imputed to me. *Secondly*, I have discussed the alleged existence of cancers having no cancer-cells. And *Thirdly*, the question whether cancer was curable. The first point has never been seriously contested, and the curability of cancer in certain cases does not appear to me to have been refuted. As regards the question of the specific nature of the cancer-cell, I am not competent to solve it in a microscopical point of view. I have only attacked it clinically. The recurrence of tumours containing no cancer-cells has convinced me that the cell is not the pathognomonic sign of cancer. M. Robert has contested my facts, and asked how I knew that it was a cancer by resting my opinion on its recurrence. Let me, once for all, answer the question.

Its non-recurrence is to me a proof that a tumour is not cancerous only when this fact is found existing in conjunction with other cir-

cumstances possessing an equal negative value. I have always founded my diagnosis on the combination of these negative characters. I admit that I may have been sometimes deceived, but how can I have been constantly deceived throughout the numerous series of cases? M. Robert has spoken of the retraction of the nipple not being a pathognomonic sign of cancer, but on what grounds does he allege that M. Robin's two cases of mammary tumours were not cancers? You answer,—On the absence of cancer-cells. But I knew both cases, one of which was my own, and in it the malady recurred two months and a half after the operation. How can this be said not to have been cancer, or how can confidence be placed on the absence of the cell as a diagnostic mark? No one has pretended that retraction of the nipple *alone* constituted a specific mark of cancer; for how can we diagnose a malady by a single character?—*Monthly Journal of Med., Dec.* 1854, *p.* 550, *and Feb.* 1855, *p.* 167.

10.—*On Necræmia.* By Dr. C. H. Jones, F.R.S.—This term is applied by Dr. Williams to that condition of the blood, in which it appears to be itself primarily and specially affected, and to lose its vital properties. It is, in fact, death beginning with the blood. The appearance of petechiæ and vibices on the external surface, the occurrence of more extensive hemorrhages in the internal parts, the general fluidity of the blood, and frequently its unusually dark or otherwise altered aspect, its poisonous properties, as exhibited in its deleterious operations on other animals, and its pronenesss to pass into decomposition, point out the blood as the first seat of disorder; and, by the failure of its natural properties and functions, as the vivifier of all structure and function, it is plainly the medium by which death begins in the body. The blood, the natural source of life to the whole body, is itself dead, and spreads death instead of life. The heart's action is faltering and feeble; the atonic vessels become the seat of congestions, and readily permit extravasations. The brain, insufficiently stimulated, after slight delirium, lapses into stupor; the medulla no longer regularly responds to the *besoin de respirer;* and the respiratory movements become irregular. Muscular strength is utterly lost; offensive colliquative diarrhœa, or passive intestinal hemorrhage often occurs; sloughing sores, or actual gangrene of various parts are easily produced; and putrefaction commences almost as soon as life is extinct. The track of the superficial veins is marked by bloody stains; hypostatic congestion takes place to a great extent; the blood remains fluid, and stains the lining membrane of the vessels. Rokitansky describes the blood as often foamy, from the development of gas, and of a dirty red raspberry-jelly colour; its serum dark from exuded hæmatine; and its globules swollen up by endosmosis. Coagula are either totally absent, or are very soft and small. The exudations are of a dirty red—turbid, thin. There is scarcely any rigor mortis; the tissue

of the heart and of other organs is flaccid and softened, and stained by imbitition of the serum. Gas is quickly formed in the vessels and in the areolar tissue, giving rise to a kind of emphysema. It is very remarkable that this necræmic condition, or one closely resembling it, may be brought on by violent shocks inflicted on the nervous system, as well as by the introduction of miasmatic or animal poisons into the circulation. Violent convulsions, overwhelming emotions, the shock of an amputation, a stroke of lightning, even a severe exhausting labour, are mentioned by the German pathologist as having produced this effect. More common causes, however, are malignant scarlatina and typhus, yellow fever. the plague, and the disease called glanders. It may be said, generally, that the early appearance of sinking and prostration in any fever, indicates that the blood is thus seriously affected. We are ignorant what is the exact nature of the changes which take place in this condition of the blood. Probably they are more of a vital than merely chemical kind—that is, they affect the properties of the blood more than its composition. The blood globules do not appear to be destroyed; but they circulate probably some time before death, as so many dead particles prove to be enlarged and to stagnate in the capillaries, and to part with their contained hæmatine. The fibrine is in great part destroyed; but how this comes to pass we are ignorant. We can perceive, on the whole, scarce anything more than that the powers of vital chemistry rapidly decay, and those of ordinary chemical affinity supply their place.—*Association Journal*, *Dec.* 22, 1854, *p.* 1144

DISEASES OF THE NERVOUS SYSTEM.

11.—ON THE NERVOUS SYSTEM.

By Dr. Marshall Hall, F.R.S., &c.

The cerebrum, the cerebellum, and, in a certain sense, the medulla oblongata, may be regarded as the Centre of the Cerebral System, of the nerves of special sense, and of volition, of intellect, and of the passions, of pleasure and of pain. By its means we are placed in relation to all that is *external*, *psychically*. By its means we perceive, by its means we seize and use, external objects.

There is good reason to believe that intellect is seated high, the emotions lower, and the faculty of pleasure and of pain lower still, in their general cerebral centre.

Below the centre of the cerebral system is that of the spinal system, and its analogue in the articulata; &c. The spinal centre includes the medulla oblongata and medulla spinalis. It may be divided into—

1. The Medulla Oblongata, the spinal centre principally of the acts of ingestion and of egestion, by which the preservation of the individual is effected, and especially those of deglutition, respiration; &c. With this centre, the pneumogastric, the diaphragmatic, &c., of such momentous importance in these acts, are essentially linked;

2. The Brachial Enlargement;
3. The Femoral Enlargement; and
4. What, I think, may be designated the *Lower* Medulla Oblongata, the analogue with its nerves of the organs of generation, of the upper with its pneumogastric; and the spinal centre in the acts for the perpetuation of the species.

In each of the acts of ingestion and egestion to which I have alluded, there is the essential agency of a diastaltic spinal arc. The pharynx and larynx are excited by eisodic and exodic branches of the pneumogastric; the pneumogastric is the internal excitor of inspiration, acting through the spinal centre and the diaphragmatic and intercostals.

Similar diastaltic nervous arcs constitute the nervous media through which emission, conception, parturition; &c., are effected. In all these are nervous *origins* in exciting surfaces, an eisodic nerve, the spinal centre, an exodic nerve, and muscular terminations. The *law of action*, the mode of action, the channel of action, are in all identical or similar. All have the same object or objects—ingestion, retention, egestion, exclusion.

Can anything be more simple and scientifically beautiful?

All these acts are *physical*. As the cerebral acts depend on the immortal ψυχη, these depend on the *vis nervosa;* as those relate to external objects psychically,—viz., to objects *without*, these relate to them physically, acting *from without to within, and from within to without.*

The spinal centre, with its nervous arcs, and the muscles in which these terminate, are the dynamic agents which accomplish that "*tourbillon*" so eloquently described by Cuvier.

The ganglionic system relates to all that is *within*, governing the *peri*staltic actions, assimilation, the secretions, the excretions, &c.

But of these functions I must treat as cursorily as I have done of the cerebral system.

They are greatly under the influence of emotion, and of sympathetic or synergic action through the spinal centre. We have only to observe the effect of derangement of the stomach, or of eroded viscera, on the action of the heart, the skin, &c., in connection with experiment, to arrive at this conclusion.

The experiments to which I allude are the following:—Let the head be removed in a frog, the spinal marrow remaining, and the circulation be ready to fail: if we now crush the stomach or a limb with a hammer, the action of the heart ceases. Let the conditions be the same in another frog, with the addition that the spinal marrow is also removed: in this case, no influence is perceived, on crushing a limb or the stomach, on the action of the heart. Now, the difference is the presence or absence of the spinal centre. This experiment, however, requires careful repetition.

[The *Spinal System* is the source and seat of the class of *Convulsive diseases.* Dr. Marshall Hall commences an interesting lecture on this subject by saying:]

I must commence my explanation by drawing your attention to three most important physiological facts:—

If we puncture or lacerate, or otherwise injure the cerebrum or cerebellum, or the cerebral nerves of special sense, in every possible manner, we observe *no results, no phenomena*, no expression of pain, no excited movements.

The cerebral system in all its parts is, in this respect, *in*excitable.

But if we touch any part of the spinal system, and especially the spinal centre, with the point of a needle, ever so slightly, there are immediately excited muscular movements, spasm, convulsion.

The spinal system is, in all its parts—*excitor, excito-motor.*

These important facts constitute the foundation of all our knowledge of the diseases of the nervous system, and are the very source of all *diagnosis* in regard to them.

For the same facts, as is proved by clinical observation, hold in regard to the human subject.

By experiment and by observation, then, we are led to these conclusions:

No lesion of the cerebral system, limited to the cerebrum, *can* be attended by spasm or convulsion.

No structural lesion of the spinal system, short of destruction, *can* occur *without* exciting spasm or convulsion.

If, in affections of the cerebral system, we observe spasm or convulsion, it is because it is *not* limited, in itself or in its effects, to the cerebral system. Thus, congestion of the centre of the cerebral system may, as in hanging, become extended to that of the spinal system, and then spasm or convulsion supervenes.

Or affections of the spinal centre may consist in gently applied *pressure*, not in lesion of tissue, and then paralysis, and not spasm or convulsion, will be observed. Or it may consist in sudden or violent *shock*, or utter destruction, and then, I need scarcely say, that paralysis, and not spasm or convulsion, will occur. All this we have seen demonstrated by experiment; all this you will see hereafter in your observation in the clinical, and especially the surgical ward.

What a means of *diagnosis*, then, have we obtained by these simple physiological facts! How has physiology become our guide in practice!

I have thus explained to you how disease of the cerebral centre may, by pressure downwards, affect the spinal centre. But another question arises: How does a disease of the spinal system, as a pure convulsive malady, affect the cerebral?—for such is the frequent event, as we observe in epilepsy.

Observe what occurs in the most *marked* cases of this dire malady. The head becomes fixed, or there is torticollis, by the action of *the muscles of the Neck*,—trachelismus; the jugular and other veins of

the neck are compressed; the capillary system of the neck, the face, the intra-cranial structures congested; the veins start, the arteries throb; the *cerebral* centre becomes implicated. *All is intelligible! All is explained!*

Affection of the centre of the cerebral system is extended to that of the spinal system—the medulla oblongata—by downward pressure: affections of the spinal system are extended to the cerebrum, by *trachelismus!*

That *all* this is so, is demonstrable. That it is *always* so, is, perhaps, what I ought not, however disposed to do so, to assert. And does it not constitute a beautiful specimen of the *physiology of disease?* But such is the whole of our recent views of epilepsy and of the *Class* of convulsive diseases, as observed in the clinical ward, to which I must now recur.

And first, pray observe with me an experiment or two.

I have removed the cerebral centre in this frog. Its spinal system is still most lively and energetic. I take and rub the toe between my thumb and finger; you observe the almost convulsive movements produced: I now touch the upper part of the spinal centre with this needle; the animal is thrown into the most violent convulsions!

These experiments are *types* of disease; they are the types of epilepsy and other convulsive diseases.

The *first* is the type of *ex-centric* epilepsy;

The *second* is the type of *centric* epilepsy.

For the first was induced by an excitant applied to a part of the spinal system at a distance from its centre; the second to that centre itself.

So it is in epilepsy; so it is in convulsive diseases generally, in the human subject! Do not these simple and illustrative facts interest you? They are of the deepest interest.

Do you not now perceive *how* dentition, and gastric and enteric and uterine irritation, may excite spasm or convulsion, even epilepsy? And do you not now perceive that ex-centric epilepsy is no more an *opprobrium medicorum* than infantile or puerperal convulsion; for that it is equally intelligible?

You see again the violent convulsions which I excite by again touching the spinal centre in this frog by this needle.

Such is the dire state of things in centric epilepsy, whether this be of organic origin, or have been organic in its course; whether it be congenital, on one hand, or inveterate on the other.

The cases of epilepsy which occur in private practice are, for the most part, of the former or ex-centric kind, and *curable*, however, for many reasons, difficult of cure. The cases which we see in the lunatic asylum, and especially in the workhouse, are generally centric, and—must I not add?—too generally incurable.

Allow me to draw your attention to this *Table*. I has cost me much labour, but it contains in a page, a volume. You will find it

in the 'Lancet,' July 20, 1850,—(*see Retrospect*, vol. xxii., p. 36.) Procure it, or copy it, and study it well. For, allow me to say, I know of no such specimen of this most interesting pathology. The *chain* of events in epilepsy and other convulsive diseases, is pursued in it, *link by link*. The *causes* are arranged in this first column, the the principles and modes of treatment in this last; the *rationale*, the various stages, forms, and complications in the intermediate ones. It would occupy us too long to enter into its details. I have only time for a rapid sketch.

The ex-centric causes act on eisodic nerves, thence on the spinal centre, and thence along exodic nerves, on the various muscles. Of these, few, many, or all may be affected, and the malady may be the very slightest or the very direst. Amongst the rest—I was about to say, chief amongst the rest—are the muscles of *the Neck and of the Larynx*.

By the contraction of the muscles of the neck the veins of the neck are compressed, as I have already noticed; and the extra-cranial and intra-cranial tissues and organs become affected with venous congestion and all its consequences. Nay, I am disposed to say that, in every case in which there is such venous congestion of the neck, face eyes, brain, it arises from this trachelismus, latent or evident. I do not wish you, however, to adopt this opinion without examination; but rather to subject it to a most careful investigation for yourselves.

By the contraction of the muscles of the larynx in laryngismus, this vital inlet to the respiration, combined, as it usually is, with *breath-struggles*, I believe the direst form of epilepsy, with its direst effects, as coma, mania, dementia, &c., to be produced.

I do not say that these effects may *not* arise in cases in which there is only trachelismus and no laryngismus; but I am convinced that they are *chiefly* the effects of laryngismus; and hence they have subsided in cases in which the laryngismus has been disarmed by tracheotomy.

And now you perceive in what sense epilepsy may be regarded as a cerebral disease. Cerebral in its very *origin* it can never be. It may be intra-cranial in its origin, because within the cranium there are many tissues, as the membranes, from which eisodic nerves arise, many eisodic nerves, the fifth especially, pursuing there a part of their course; and the medulla oblongata,—all which may be excited by the presence of an exostosis, a tumour, a variety of diseases. But I repeat that no disease of the cerebral centre, limited to that centre in itself and its effects, does or can produce epilepsy.

Exciting causes: the Spinal system; the Neck; the encephalon; such are the links of this chain of pathology. There remains another to which I must now draw your especial attention: it is—the medulla oblongata, with its *pneumogastric nerve*.

I have already adverted to laryngismus as giving to epilepsy, and and convulsions generally, its most formidable character. This form of laryngismus is *spasmodic* or *convulsive*, excited through the spinal system.

But there is another form of laryngismus. After a severe epileptic convulsion, the patient is left in a state of *coma*, or, as it may be termed, of *simple apoplexy*. This condition may prove fatal. The respiration becomes stertorous—that is, there is laryngismus. But this laryngismus is very different from that formerly described: it is less complete, but more persistent; it is, indeed, permanent; some one said, in rather familiar phrase, "The patient snores his life away." This laryngismus is *not*, like the former, spasmodic, but paralytic. It is owing to compression or congestion—an apoplectic state—of the medulla oblongata, and *paralysis* of the pneumogastric nerve, in which every branch, as well as the recurrent laryngeal, is implicated.

Apoplexy of the medulla oblongata; paralysis of the pneumogastric nerve!

Legallois—the illustrious Legallois—divided the pneumogastric above the origin of the recurrent. The larynx became partially closed. The breathing was *stertorous*. In a word, every phenomenon was obscured which I am about to describe to you as occurring in the coma or simple apoplexy observed after a severe epileptic seizure.

And what do we observe? We observe paralytic laryngismus, impeded respiration, augmented apoplexy; we also observe—or, rather, I have observed—*a diffused bronchial rattle*, the effect of *paralysis* of the bronchial and pulmonary branches of the pneumogastric!

Here a most interesting phenomenon presents itself. The cerebrum is not essential to *life*. How then does apoplexy, simple, cerebral apoplexy, destroy life? It does not do so. Apoplexy only destroys life when, by its congestion by downward pressure, it implicates the medulla oblongata and its pneumogastric nerve. It is by the apoplexy of this medulla and the paralysis of this nerve, that life is, in effect, destroyed!

Generally the patient dies of paralytic laryngismus, or of laryngeal asphyxia. From this danger he is rescued by early tracheotomy.

But if tracheotomy *be* performed, but performed too tardily, the patient, however saved from *laryngeal* asphyxia, dies of *bronchial* asphyxia!

There are other branches of the pneumogastric nerve—the cardiac and the gastric. The heart and the stomach participate in the paralysis of the general pneumogastric.

The observant physician will observe palpitation as a symptom in some cases of epilepsy. This is explained by excitement of the cardiac branches of the pneumogastric.

In other cases, there are hiccup, eructation, acidity of the stomach; the effects of excitement of the gastric branches of this composite nerve.

As in these cases there is morbid *action* in the heart and stomach, so in deep epileptic coma there is *failure* of the powers of these organs.

The pneumogastric nerve is not more involved in epilepsy than in other convulsive diseases. All the phenomena just described as

observed in the former, are again met with in the latter, only, of course, modified by the exciting causes, and the conditions of age and sex.

In infants there is the same or similar action of the muscles of the neck, and of the larynx and respiration. In puerperal convulsions the symptoms are identically those of epilepsy.

There is a form of epilepsy which I have not yet noticed. It is the *epilepsia syncopalis*. The patient, instead of turning purple, turns pale and ghastly. Sometimes this syncope is *fatal!*

So it is in the laryngismus stridulus of infants. The little patient not unfrequently dies suddenly—too suddenly to be the effect of asphyxia. The case is cardiac, syncopal.

Both these cases are affections [*shock?*] produced through the medium of the pneumogastric nerve.

But, you remember that I spoke, in my lecture of yesterday, of a *lower* medulla oblongata, and of a *lower* analogue of the pneumogastric. These, too, are influenced by the convulsive seizure.

The laryngismus stridulus is frequently excited by enteric irritation. This is also frequently attended by fret of the bladder.

Epilepsy is frequently induced by enteric and uterine irritation, and is sometimes attended by involuntary evacuations of the bladder, rectum, &c.

The urine is frequently morbid in both cases.

There is something in all this *analogous* to the modes of action of the medulla oblongata and the pneumogastric nerve.

It is obvious that both these portions of the nervous system form ONE. Both are liable to be affected in one and the same convulsive seizure.

Perhaps the *lower* portion of this system is most clearly isolated in what relates to *Obstetrics*. To excite the cervix uteri is frequently to excite abortion. To excite the cervix uteri during parturition is to excite the uterine efforts. But to excite the cervix uteri during parturition is, also, frequently to excite a puerperal convulsion! These are facts familiar to every experienced practitioner. They are *proofs* that "a meddlesome midwifery is bad."

Other facts of the same kind are constantly elicited in practice: to excite the rectum, by the pressure of the fingers backwards, excites uterine action; and it is well known that the excitement of the internal surface of the uterus by the hand, introduced in certain forms of hemorrhage from inertia, excites the uterus to energetic contraction.

All this is accomplished through the *lower* medulla and its analogue of the pneumogastric.

I think that a subject very inscrutable hitherto will receive elucidation from these studies—that of sterility, inertia of the uterus, &c. &c.

[Dr. Hall proceeds to give us some interesting remarks on the *treatment of Convulsive diseases, and Epilepsy in particular.*]

The excitements, the irritations must be, as far as possible, avoided. The morbid conditions of the blood must be remedied. A low posture and deep sleep must be carefully shunned. The neck must be cautiously guarded against a tight collar.

One event we must constantly suspect: an undue acidity in the stomach. Against this, the bicarbonate of potass is the present remedy, and should be taken on every recurrence of a threatening symptom. Many patients have carried about with them a solution of twenty-five grains of this bicarbonate in two ounces of water, and taken it in such circumstances with the greatest advantage.

Another event to be suspected is the formation and retention of scybala in the colon. These are only effectually removed by an enema of three pints of warm water, slowly administered. But in this manner, too, the threatened attack has been averted.

One patient succeeded in replacing an attack by a fit of vomiting, taking for that purpose, on any threatening, half a drachm of ipecacuanha. To this I have sometimes *added* an equal quantity of the bicarbonate of potass.

The bowels should be kept well relieved daily; but I have known a dose of purgative medicine constantly to produce a seizure.

One patient took many remedies empirically. None did good. But the turpentine acted injuriously.

Of all our remedies, none is more important than a well-regulated, simple, digestible, nutritious diet.

Against the undue excitability of the spinal centre in epilepsy, well-appointed walking exercise is the specific remedy. But *effort* or *fatigue* adds to the evil.

I think there is as the cause of sleep a latent trachelismus. The usual posture during sleep is the recumbent. Both these must conduce to the epileptic seizure. I have constantly recommended a posture during sleep at an angle of fifty degrees. And I think that I have seen advantage from gentle movements and noises in the patient's room during the first sleep, to prevent deep sleep.

All empirical remedies may be safely discarded. It is very improbable that *any* individual agent should be able to accomplish all that is required in a case of epilepsy. We may well ask, now that this malady is somewhat understood, why epilepsy should be treated empirically more than apoplexy, or than infantile or puerperal convulsion?

It has appeared to me that the hyoscyamus may allay the nervous excitability so frequently observed in epilepsy. As a little spirit obviously does good in the undue cerebral excitement of delirium tremens, I have thought that minute doses of strychnia may be useful in the undue spinal excitability in epilepsy. For the organic changes induced by epilepsy, mercury is the remedy.—*Lancet, Nov.* 11, 18, *and Dec.* 2, 1854, *pp.* 392, 411, 451.

12.—ON THE USE OF COTYLEDON UMBILICUS IN EPILEPSY.

By Dr. E. H. Sieveking, Assistant Physician to St. Mary's Hospital.

[The active principle of this drug has not yet been discovered, but would seem to be analogous to digitalis, from its slightly sedative and diuretic action. The only organs acted upon by it appear to be the kidneys. In the first case of epilepsy reported by Dr. Sieveking, the patient had been subject to the fits for six years. Before giving the medicine, she had six fits in about eight days. The following was prescribed:]

℞. Liq. cotyl. umbil. ℥j. ter die., continued to Feb. 15, with one slight fit, which only lasted ten minutes.

The cotyledon was persevered in, with an occasional colocynth pill, and a little cough medicine, to the 29th of March, only one fit having taken place during that period. On the 6th she was discharged, but the cure was not permanent; for, on the 19th of April, 1853, she returned, stating that the fits had returned the previous week, and that seven paroxysms had occurred since.

The liq. cotyl. umbil. was again ordered in the same dose.

May 3. One fit since last night. Pergat.

20th. No fit for a fortnight. Pergat.

June 11. One fit since last visit. Pergat.

July 5. One fit in the night, the week before last, from excitement; none since.

Electuar. ferri ℥j. ter die.

12th. Pergat.

25th. One fit since last visit.

℞. Nitr. arg. gr. ss., extr. gent. gr. iv., pil. ter die. sumenda.

Aug. 2. Had a fit during the last visit, lasting a short time, and a slight fit last night. Rep. pil.

16th. Has had two fits. Rep. liq. cotyled. umbil. ℥j. ter die.

25th. No fits. Pergat.

Sept. 28th. One fit since Aug. 25. Pergat.

After this there was no return of fits during the whole period she continued to take the medicine; she also lost what she called the slight attacks—brief, momentary fits of vertigo and semi-unconsciousness, and was discharged, cured, on Nov. 18th. The cure, however, again was not permanent; and the last notes of the case, I regret to say, appear to show that the cotyledon had lost whatever efficacy it may previously have possessed. She returned in Jan. 1854, after a free interval of three months. She was now pregnant, and continued to have the fits up to the period of delivery, without relief from the cotyledon.

Case. 2.—Aug. 2, 1853. Thomas C., aged 44, a widower and messenger, has for three days past been subject to dizziness. Three days ago there was loss of consciousness, and paralysis of the hands for a time.

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ter of an hour, of which there is now no trace. There is pain in the forehead; pulse 84; tongue furred. On Sept. 3 he had a fit, in which he lost his senses for about half an hour. Two fits occurred again between Aug. 19th and Sept. 23rd; another on Sept. 25th, lasting ten minutes. The treatment up to this time consisted mainly in counter-irritation and purging, the symptoms being regarded as indicating a congestive, if not inflammatory condition of the brain.

Oct. 7. Last Sunday (five days ago) his hand became pale and "drawn;" there was a sensation passing up to the head, and he then fell down in a fit, and remained unconscious for about ten minutes. The same symptoms have occurred once a-week. He was recommended to apply a ligature round the arm, and tighten it as soon as he feels the commencement of the aura, and take liq. cotyled. umb. ʒij. ter die.

Oct. 14. States that he has derived a great deal of good from the last medicine. Pergat.

21st. Has a sort of cramp in his hands when he wakes up in the morning, with a "tapping" on the head. Pergat.

28th. Can't get rid of the ticking in the head; complains of nothing else, except a slight increase of deafness. Says the cotyledon umbilicus acts as a diuretic. Rep. cotyled. umbilicus, empl. vesicat. auricul.

Nov. 11th. No fit had recurred since the first employment of the cotyledon umbilicus. I have not seen this patient since.

Case. 3.—This case might have seemed a peculiarly favourable case for the employment of cotyledon umbilicus, from the apparent absence of all complications; but it failed, like the various other remedies which were employed, in effecting any permanent impression. It was that of an intelligent lad, aged 17, who. for three years before coming under treatment had been subject to fits, occurring on an average once or twice a month. There was no hereditary or other taint traceable; masturbation was absolutely denied by the father, who had closely watched his son, and the former attributed the malady solely to over-reading. The cotyledon was given alone, or with taraxacum, from Oct. 18th to Nov. 29th; and although the headache of which he complained was relieved by a seton, the monthly fits were not postponed, and have of late even become more numerous.

Case 4 was one that would be at once pronounced hopeless as regards the efficacy of any drug. The patient was a porter, aged 30, who had been subject to epileptic fits from his infancy, whose father was paralytic and brother epileptic; they were preceded by a sense of his "head going round," and occurred once or twice monthly; and, during the last twelve months, he had also been affected with choreic movements of the left arm. He took the extract of the cotyledon, first, gr. xv.; afterwards, gr. xxx., three times a-day, while under treatment, viz., from Feb. 7, 1854, to April 4, with occasional purges. During that period he had four fits.

Case 5.—March 14, 1853. W. L., a lad, aged 15, of a scrofulous habit; had no disease till the appearance of the fits, to which he has

been subject about a year and a-half. The fits commence with a scream, and the unconsciousness sometimes last two hours. They are followed by frontal headache. He has not bitten his tongue. The pulse is 80, strong; the bowels open; there are no worms; he passes much urine before the fits; he now has five to six fits daily. He never had a blow on the head as a child. Masturbation denied. Ext. cotyled. umbil. gr. xv. ter die. Mist. magn. et rhei. ℥j. alterna mane.

28th. Has had no fits since last visit; feels much better; has no headache. Pergat usu mistura et pilul.

April 7. Has had no return of fits till last night, when he had a "dreadful" fit, and another this morning. Wets his bed at night. The free interval was regarded by the mother as unusually long. Unfortunately, this was the last opportunity I had of seeing the boy; but the case, so far as it goes, may be used as evidence in favour of the cotyledon, the more so, as the patient's age and sex, as well as the duration of the illness, do not favour the assumption of any mere mental impression.

Case 6.—In this case, a young clerk, of 17 years of age, epileptic fits had occurred, with gradually increasing frequency, for a year before he came under treatment. Gradually increasing doses of sulphate of zinc, attaining gr. x., effected what appeared to be a cure; but, at the commencement of the treatment, the cotyledon was tried from April 21 to May 9, during which time he had two fits, which were said to have been less strong than the previous, though about the same rate of frequency.

Case 7.—This case was one in which, not only from the ancient date of the disease, but the probability of centric disorganization (tubercle) of the brain, and the impoverished circumstances of the patient, treatment promised to be of little avail. He was a porter, aged 18, who suffered from the first fit at the age of five. The fits had remained in abeyance till within about eight months before coming under treatment. It was commenced with the nitrate of silver, but the fits became more frequent; the silver was therefore exchanged for the cotyledon, and the previous interval was restored, and intermissions subsequently protracted longer than they had been before treatment. A severe attack of pleuritis prevented the further attendance of the patient, and he was lost sight of.

The above cases, I think, hold out some encouragement to the further employment of this certainly innocuous remedy. Whatever its active properties, much undoubtedly depends upon the manner in which the extract is prepared, as in all vegetable extracts; much must also depend, if the active principle is remedial in epilepsy, upon a proper diagnosis of the exact form of the disease to which it is applicable. A good deal remains to be done before these desiderata will be supplied.—*Med. Times and Gazette, Dec.* 2, 1854, *p.* 564.

13.—ON INFANTILE CONVULSIONS ARISING FROM EXCENTRIC IRRITATION.

By Dr. Robert Molloy.

[Dr. Molloy dissents altogether from the opinion that dentition is a frequent cause of convulsions in children. These convulsions may arise at all periods of dentition, and often without the slightest indication of irritation.]

As, therefore, these varying conditions of dentition bear no constant or real relation to the epileptic convulsions, it is of the highest importance to notice what other morbid symptoms accompany them, in order to trace out the effective cause of so much mischief. This, I believe, will be found almost invariably in the *gastro-intestinal secretions*, which are constantly at fault. My attention was first directed to this subject more particularly by the case of a child, in which I had an opportunity of watching, not only the symptoms present in these convulsive attacks, but all which preceded and followed them. I remarked on the first epileptic attack to which he was ever subjected, that while under the relaxing influence of the warm bath, an involuntary and very copious offensive discharge from the bowels took place, with *immediate* remission of the convulsive moments. On the second occasion a similar result occurred; and here the change was so instantaneous and striking, from an apparently dying condition to one of promised recovery, that it determined me to adopt the plan of treatment which I have since found singularly successful. This consists in the use of warm water injections, which may be administered to a great extent, and, even in the case of infants, more copiously than any one might believe who has never tried them. I have not unfrequently passed into the bowels twelve syringefuls (about twenty-four ounces) before they could be made to act properly, but in every case with immediate relief, as soon as even a small quantity had been injected. This I have commonly seen:—A child is labouring under fearful convulsive movements; two or three syringefuls of fluid are thrown into the intestines, and on the instant the spasms subside, although but for a short time. I have awaited their return, and again injected more, with a like result; but of necessity the patient is not permanently secure from their recurrence until the bowels have been well emptied, all the offensive discharges removed, and a more healthy condition obtained.

The fæcal evacuations in these cases *contain little or no bile*, and are usually very offensive. In children thus predisposed, a daily inspection of the intestinal excreta should take place, as a necessary part of the medical attendant's duty; for I have very frequently been enabled to foretell the approach of these epileptic attacks by the appearances they presented. An additional argument, derived from physiology, may no inaptly be mentioned here. The perspiration of infants is very trifling; their urine, too, is comparatively free from saline constituents. Now, in consequence of the law of compensation, it is probable the greater

part of these secretions may have to pass off through the intestines, rendering their contents normally more irritating than, in process of growth, they afterwards become; while the greater relative size of the liver, doing duty, as it were, for the lungs, as well as for itself, compels an incessant functional activity, and requires equally the discharge of bile into, and its speedy withdrawal from, the bowels, to maintain the complete integrity of its functions.

The notes I have taken at different times would enable me to cite numberless cases in corroboration of these statements; but I shall content myself with giving outline sketches of a few of them, in which the symptoms were either strongly marked, or had appeared after other plans of treatment had been tried.

A girl, three years of age, semi-idiotic, with congenitally malformed head, had been subject to frequently-recurring convulsive attacks, which, at that time, were referred to the irritation of dentition, and treated in the usual manner by warm baths, incisions of the gums, and application of leeches to the head. One afternoon, at a period when the child was considered unusually well, an epileptic seizure came suddenly on, without any premonitory symptom whatever. Application being made to me, I gave an emetic, which acted in about twenty minutes, ejecting from the stomach a large quantity of undigested salt beef and greens, she had been allowed to eat at dinner. Upon this there was immediate cessation of all convulsive movements; neither did they again return.

Another, four years old, the favoured daughter of highly-respectable people, who indulged her to an extent which assigned to her complete mastery over them—a child, fat and gross, with a colour in her cheeks that might vie with any country rustic's—was seized one morning, about eight o'clock, with an attack of epilepsy, the first she had ever experienced. The convulsions were certainly the most fearful I have yet seen in one so young; and I was informed that she had been drooping for a few days, was listless and sleepy, but that no aperient medicine had been given for a long time, in consequence of the resistance she made whenever it was attempted. In this instance the effect of the injections was curious and extremely interesting; three or four plunges of the syringe stilled at once, as though by magic, the violent convulsive movements, and although the effect was only temporary, it is of importance to remark, that it was invariable, and always followed by the same calm. It appeared as if the distention of the water removed the surface of the bowels from contact with any irritating matter which was present. As no liquid medicine could be administered by the mouth, I placed some calomel powders on the tongue, and persevered with injections until six in the evening, when the bowels were for the first time relieved, bringing away an enormous quantity of round masses of fæcal matter, *entirely free from bile*, and resembling nothing more nearly than the once-favoured therapeutic remedy—"album græcum." There scybala were literally

alive with thread-worms, in themselves a most fertile source of intestinal irritation. On this occurrence the convulsions remitted, and shortly afterwards disappeared altogether; nor has she to this date ever had a return.

The third case refers also to a girl, upwards of three years of age, who had completed dentition, and was the only living child of four, the others having died at an early age, either from cerebral excitement or decided hydrocephalus. She was a sharp, precocious child, so that attention would more naturally be directed to the state of the brain in consequence of this circumstance, and of her previous history. The first epileptic attack occurred eighteen months since, at a time when she had been taken a short trip into the country, for change of air, and continued for some hours after she was brought home. She was under treatment longer than usual, principally on account of the time which had elapsed before any continuous plan could be had recourse to. A second epileptic seizure, a few weeks ago, was promptly and decidedly removed by injections. Neither of these attacks differed in any respect from those previously detailed.

The many facts I have witnessed strongly incline me to the belief, that this predisposition to excentric epilepsy is hereditary; and one case in particular afforded a very peculiar confirmation of this view. A lady, who eventually became my patient, had three children, all girls, none of them of the strongest. They were sorely tried in passing through the customary diseases, but never had any convulsive attack. After being a widow for a few years, she married again, and gave birth to a boy, who suffered long and severely from convulsions. On inquiry, I learnt, not only that the father had been similarly afflicted when he was a child, but that none of the wife's or her first husband's family had ever been subject to them. This was the more conspicuous, because I have generally remarked that girls are more predisposed than boys to epilepsy.

But perhaps the real value of the plan of treatment here advocated was more advantageously displayed in the following case than in those already reported. It made a deep impression upon me at the time, not only on account of the intolerable state of the intestinal secretions in a child of such tender age, but from the immediate and almost instantaneous relief afforded by the injection, which active aperients had failed to obtain. A young child, in whom dentition was complete, was attacked one evening with convulsions, the first he had experienced, and before I could reach the house had two severe fits. A plentiful enema was at once administered, which, in the course of a few minutes, brought away an enormous quantity of extremely offensive evacuation, so that the room was hardly bearable. No return. About two months subsequently, threatening symptoms of convulsion again made their appearance. Purgative medicines were given, by which the bowels were copiously relieved; but, from the persistence of the symptoms, it was deemed advisable to employ an

enema, with the immediate effect of emptying the lower intestines of a very great quantity of slate-coloured and most unhealthy secretion. He was quite well the next day, and has continued so ever since.—*Lancet, Jan.* 20, 1855, *p.* 63.

14.—TREATMENT OF HEMIPLEGIA FROM SOFTENING OF THE BRAIN.

By Dr. J. Hughes Bennett, Edinburgh.

I believe that the most important end for the practitioner to aim at, in the early treatment of these cases, is to keep down the frequency and force of the heart's action. For this purpose the strict maintenance of the horizontal posture is of the highest moment; and when the patient is conscious, it is most desirable that the mind should be tranquillized by every means. It will, of course, likewise be necessary to remove all local impediments to the easy flow of the circulating fluid; and it is as well that the head should be slightly raised, sufficient to prevent gravitation favouring the escape of blood from th ruptured vessels, but not so as to create any impediment to the flow which might embarrass the action of the heart.

To remove any source of nervous irritation which may be operating injuriously on the brain, the bowels should be cleared; and in order that there may be as little effort as possible on the part of the patient in the expulsion of the contents of the bowels, it is expedient that this should be done by enema; but if this fail, and the vital power of the patient do not forbid it, you may give croton oil, a drop or two of which, placed on the tongue, will operate freely; or calomel, in powder, to the extent of five or ten grains, which may be similarly administered. I would advise you to limit the further administration of drugs, to giving some slight corrective, as an alkali—ammonia being, on the whole, the most appropriate—unless, indeed, you find the patient in an extremely prostrate condition, when it will be necessary to combine with it the cautious exhibition of other stimulants and restoratives, as chloric æther, brandy, &c.

The question of bleeding will arise; and, under the popular notion, that all head attacks are accompanied and caused by the rush of blood to the head, you will be pressed to have recourse to this expedient. There are three objects to be attained by bleeding; *first*, to diminish an undue amount of blood in the head; *secondly*, to check hemorrhage, or to prevent it; and *lastly*, to quiet the heart's action.

If the patient be cold and collapsed, it is clear you should not take blood; nor should you have recourse to this practice if the heart's action be very feeble or intermittent; nor if there be an anæmic state; nor if the patient be of very advanced age; nor if the evidence of extensive disease of the arterial system, or of the heart, leave no doubt on the subject; nor would it be desirable to bleed if it were clear that

already a large amount of hemorrhage had taken place into the brain. Should none of these objections exist, then you will have to consider whether any or all of the indications above named need to be fulfilled, and whether bleeding (local or general) promises to fulfil them. As to the first indication, namely, the diminution of an undue amount of blood in the brain, I think modern investigation of the actual state of that organ clearly points out, that the brain is not in a hyperæmic state, in the cases in which the form of hemiplegia under discussion is likely to occur. Will taking blood check or prevent hemorrhage? The sudden or rapid abstraction of a moderate quantity of blood, either from the arm or temple, or by skilful cupping from the nape of the neck, may, I can conceive, check hemorrhage; and with this object it is, sometimes, a very justifiable practice, but the quantity taken should not be large. Now and then bleeding helps to diminish the frequency and force of the heart's action; but here, again, the quantity of blood withdrawn should be moderate, for the removal of much blood is apt to quicken the heart's action and render the blood poor. I would have you to look upon this question, to bleed or not to bleed, as almost the most important one you will have to decide; and, judging from my own experience on this point, as well as from the results of the practice in a large number of cases collected from various sources, as I pointed out in a former lecture, I have come to the conclusion that, in cases of white softening, with or without hemorrhage, you are less likely to err by omitting rather than by adopting the practice.

You will often be consulted as to "some expedient for promoting the restoration of the paralysed limbs to their normal condition." To this question after having given a fair trial to the various means which have been proposed, I must reply, that I know of nothing which more decidedly benefits the paralyzed limbs than a regulated system of exercise; active, when the patient is capable of it; passive, if otherwise. As to the use of electricity, which is now much in vogue, or the employment of strychnia, which has been strongly recommended, I feel satisfied, as the result of a large experience, that the former requires to be used with much caution, and that the latter is very apt to do mischief, and never does good. I have seen cases in which, after the employment of electricity for some time, that agent has apparently brought on pain in the head, and has excited something like an inflammatory process in the brain. And so strychnia also will induce an analogous condition of brain, and will increase the rigidity of the paralysed muscles.—*Monthly Journal of Medicine, March*, 1855, *p.* 244.

15.—ON THE TREATMENT OF APOPLEXY.

By Dr. R. B. Todd, F.R.S., Physician to King's College Hospital.

[The following constitutes Dr. Todd's directions with regard to *bleeding* in apoplexy:—]

The treatment of patients with apoplectic symptoms must not be regarded as a matter of routine, but as a question of grave import, and which demands the most anxious consideration of the practitioner. Let me add, that it sometimes requires the exercise of no small courage and self-possession to resist adopting that practice; for the popular feeling, led by a formerly too prevalent medical practice, is entirely in favour of it, and would readily condemn a practitioner as guilty of the death of his patient who suffered him to die unbled. It is a far more dashing and courageous thing to open a vein on the spot, and in the presence of a number of anxious friends, than to adopt less showy, and apparently less active, measures.

But, indeed, you need not be inactive, even if you decide against adopting the plan of bleeding. Having placed your patient in an easy position, in which no excitement of muscular action is likely to take place (for you must bear in mind that reflex actions may often be readily excited in these apoplectic cases), you should immediately direct your attention to the state of the stomach and intestinal canal. Sometimes in these cases the stomach is overloaded, or the bowels are confined, and the administration of a quickly acting emetic, or even of some purgative medicine will often provoke a moderate sickness, which unloads the former. Nor can there be any objection to adopting measures to clear out the bowels, either by an active purgative administered by the mouth, such as calomel or croton oil, or by a stimulating and purgative enema, or both.

If upon full inquiry into all the particulars of the case, you find that your patient is of full plethoric habit, with too much blood in his body, and with a sufficiently strong heart, you may bleed him with every chance of success; but if he has been of intemperate habits, is labouring under organic disease of the heart and arteries, is of gouty or rheumatic constitution, then, whatever popular or medical custom may say, my advice to you is, hesitate much before you deplete by bleeding.

The objects which it is proposed to gain by bleeding, are, a diminution of the cerebral congestion, and the stoppage of the hemorrhage into the brain; and where it is quite clear that cerebral congestion does exist, and that that congestion causes the cerebral hemorrhage, this is clearly a rational practice. But you must bear in mind that in a large number of the cases—probably the majority—there is in reality no cerebral congestion, and that the hemorrhage is of a kind not likely to be stopped by taking away blood—by establishing another hemorrhage elsewhere.

On the whole, then, I think that the results of experience denote that the majority of cases of apoplexy are best treated by purging, shaving the head and keeping it cool—perhaps blistering, and that bleeding is rarely applicable, except to the young, vigorous, strong, and plethoric.—*Monthly Journal of Med., March*, 1855, *p.* 239.

16.—*On Cerebral Softening.* By Dr. R. B. Todd, F.R.S.—In looking into the pathology of this condition of the minute blood-vessels, you may fairly ask, is it, indeed, the primary and essential disease? or is it not rather an effect of a depraved nutrition of the tissues to which these canals are the carriers of nutriment? Now, a little reflection would lead you to say that both these questions may be answered in the affirmative, and the evil of which we speak may either commence in the brain-tissue, the brain-fibre, or the brain-cell, which consequently cease to draw upon the blood-vessels to their wonted extent; and the blood-vessels, therefore, deliver to them their nutrient matter in diminished quantity; their activity is, consequently, proportionably diminished, and an atrophic state ensues. Or the evil may commence in the blood-vessels; and this will probably be the favourite view. We know that the larger vessels are the seat of numerous atheromatous deposits, and not only the cerebral vessels but the aorta itself, and the radial arteries, as we can often learn by feeling the pulse in the living individual. Why should not this atheromatous state extend to the minutest vessels? I do not express any strong opinion in favour of either of these views; but I would say that it seems to me that pathologists do not enough regard the tissue itself as being the starting point of morbid change; and are too apt to overlook the fact that the power of attraction of the tissue for certain elements of the blood, is not the least important agent in determining the greater or less flow of blood to the organ or tissue.

And, indeed, whichever of these two views we adopt, who will say that in the blood itself we do not find the chief source of evil, which may tell either upon the blood-vessels or upon the elements of the tissues?

Without waiting to decide upon this point, we may, then, lay it down that, whether in consequence of the disease of the blood-vessels or otherwise, the nutrition of the brain becomes impaired, and this shows itself mainly in altered consistence of the nervous matter. Its colour does not undergo any appreciable alteration, and if you look at a portion through the microscope you will not detect any obvious change, the vesicles remain the same, and the fibres remain unchanged; but, as I said, the consistence is diminished, instead of being firm, the tissue of the organ has become soft and pulpy, and in some cases almost diffluent and of the consistence of cream. The diseased blood-vessels lie in the midst of this pulpy mass for some time without undergoing any further change, but sooner or later under some mental emotion, or during some increased heart's action depending either upon mental emotion, upon derangement of the digestive organs, some bodily exertion, or increased mental effort of any kind, the blood is sent with undue force, or in unusual quantity into the vessels, and in consequence the vascular canals in the pulpy portion of the cerebral tissue being deprived of their usual support, give way, and blood is effused into the softened part of the brain, which it breaks up, and the

more readily in consequence of its already diminished consistence. This is the *rationale* of the development of many an attack of apoplexy, from which the patient may or may not recover, according to the extent of brain previously softened, and according to the amount of blood effused.—*Monthly Journal of Med., March*, 1855, *p.* 240.

17.—ON HYSTERIC DISEASES.

By Frederic C. Skey, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

[We all know that with respect to hysteria the diagnosis is sometimes difficult. It is often as difficult to say what a certain disease is *not*, as to say what it really *is*. Of this singular disease, Mr. Skey gives us some good examples:—]

Eliza N., aged seventeen, a healthy looking girl, by occupation a domestic servant, was admitted into Treasurer's ward on Oct. 25th. She had been suffering from severe shooting pain in the left gluteal region. The slightest touch upon, or movement of, the joint, caused the most excruciating pain, and she complained especially of pressure over the insertions of the psoas and iliacus muscles. She was quite unable to walk; gentle application of the hand gave severe pain, but on Mr. Skey taking the limb up and raising it gently, while directing her attention to other subjects, she uttered no complaint, and the pressure was then made forcibly in every direction without causing suffering. Ordered common diet. Fluid extract of opium, two drachms; compound soap liniment, one ounce and a half: make a liniment to be applied to the hips. Citrate of iron and quinine, eight grains; tincture of orange, one drachm; in water, twice a day. Subsequently some tincture of valerian was added to the above, and she was discharged on Dec. 5th, able to walk perfectly well.

Emma Y., aged nineteen, was admitted on Nov. 19th last, into Lucas ward, suffering from supposed disease of the hip-joint, in which she remembered to have had considerable pain for many years, but which had recently increased greatly in severity. She had been treated for this disease by a great variety of applications, but without relief. She menstruated last in the month of May, the catamenia being for the above period entirely interrupted. She complained of severe pain on the slightest movement of the limb, and expressed by her countenance great apprehension at the slightest examination. On applying the hand to the surface over the joint she shrank, but shortly allowed Mr. Skey to take the limb into his hands, and considerable pressure was made around and over the articulation, without occasioning additional pain, nor was any caused by rather violent friction of the hand over the trochanter. Mr. Skey considered the malady to be that of hysterical neuralgia. She was ordered aloes and

myrrh pill, ten grains, every other night. Fluid extract of opium diluted in compound soap liniment, to be rubbed over the entire joint, and compound iron mixture was ordered three times in each day. She was placed on meat diet and allowed a pint of porter daily. Under this treatment all symptoms of local pain subsided in three weeks, and she walked about the ward without lameness.

Caroline W., aged thirty-three years, a pale, emaciated woman, was admitted into Lucas ward on Nov. 9th. She had been confined about six months previously, and had never fully recovered from the *debilitating effects of a profuse hemorrhage which followed her delivery.* Her thigh was bent at a right angle to her body, and turned over towards the opposite side. It had been in this state for about a fortnight, but had been gradually contracting for more than a month.

She complained of most intense pain on the slightest pressure in the region of the joint. A week after her admission she was placed under the influence of chloroform, and the leg was gradually, but cautiously, brought down to a straight line, and bandaged to a side thigh-splint. At the same time she was ordered ammoniated citrate of iron, eight grains; tincture of valerian, one drachm, in an ounce of water, thrice a day; wine, four ounces daily.

After being kept in this position for about five days, the bandage was taken off, and the leg remained in the natural position, without the slightest tendency of the muscles to displace it. She was discharged on Dec. 1st, complaining of some slight pain, but greatly improved in health and condition.

It is quite astonishing with what exactitude the details of true affections are represented, not only by the presence of pain and other concomitants, but by the physical manifestations of the disease imitated. In their treatment I have no hesitation in expressing my belief that they are too frequently confounded, and means the most unsuitable and unappropriate are resorted to for the cure. There is no locality more fruitful of this error than that of the spine. Within twenty years of the present time, our sea-bathing places of resort were crowded by hundreds of young women who were confined to the horizontal or semi-recumbent posture for years, were excluded from society, debarred their education, restricted in their natural food, and compelled to adopt the miserable substitute of a medicated diet for years, simply because a hot sponge created a sensation of uneasiness, or, if you prefer it, of pain, at a given vertebra, when passed downwards along with the spine, or on some other frivolous evidence of actual disease. Now true disease of the spinal column is very rare in any class of society; but these cases were one and all cases of hysteria—cases of impaired health in young women, varying in age from seventeen to twenty-five, or older—operating on constitutions, of which temporary debility, coupled with permanent excitability of the nervous system, were the especial characteristics. Diseases of the spine were the rage and fashion of the day.

Let us examine for one moment the value of this evidence which sanctions treatment so large—both physically and morally, so injurious. By disease of the spine, we understand disease of the spinal column. This column is, except under very unusual circumstances, the subject and seat of but one disease—viz., caries, which consists of a softening, and subsequently a crumbling, of the bodies of the vertebræ. The loss of substance to the upright column, in persons whose health admits of exercise, is necessarily followed by curvature and loss of height to the person. The spinous processes project backward, as you are well aware, and angular curvature is permanent. Now, there are two most important pieces of evidence to be adduced against the presence of such a condition of the vertebral column in these cases. The first is obtained from the fact, that pain, correctly so termed, is not a concomitant of the true disease; whereas, in the factitious form of the disease, pain is, in truth, the only one indication. If you examine the spine of an hysteric young woman, something more than moderate pressure will rarely fail to obtain an expression of suffering somewhere; and to a medical man who has cudgelled his brain for months, to discover the real cause of his patient's ailments, this small expression, whether of pain, or whether of mere discomfort, will suffice—a disease of the spine is proclaimed to the parents, and the result we all know too well.

Let me give you a practical hint on the mode of examination of the spine of a young person. Call to your recollection the last case you saw examined. The patient, probably a young female, was placed on a bed on her face—or rather, it should be said, she placed herself. Her arms are bent at her side, so that the weight of the chest is mainly sustained by the forearms. The consequence is, that the dorsal vertebræ are not in a continuous line with the lumbar, but rise upwards gradually towards the head. The consequence of this attitude is, that an angle is formed at the lumbar part of the column, which causes a strain upon the anterior vertebral ligament, and I defy any person, male or female, to suffer pressure on these lumbar vertebræ, in such an attitude, without more or less, if not of positive, pain, at least of the apprehension, which causes an expression of suffering. When you examine the spinal column, for this or any other purpose, lay your patient perfectly flat on a hair mattress, with the arms extended from the body, or let her sit or stand upright.

The second form of evidence is this: there is an anachronism in the periods of the two diseases, the true and the false. We rarely find caries of the spine to appear for the first time long after puberty—rarely after the age of sixteen or seventeen; while the putative malady—the factitious disease—rarely appears till after the age of seventeen; and what is yet more remarkable is, that the spine always, eventually, wins the victory, and the patients recover without the slightest evidence of a curved spine!

Bearing on this subject of supposed diseases of the spine was a case I recently saw in private. A gentleman, aged about thirty, after a residence of many years in the West Indies, returned to England, and during last winter was for some days exposed to severe cold. Shortly afterwards, while making a spring, he was seized with severe pain in the neighbourhood of the third and fourth lumbar vertebræ, and was confined to his bed for a few days, when he resumed his out-door occupations, but with the recurrence of pain in the same locality towards the end of each day. In the summer, he had a return of the former pain, and two issues were placed in his back opposite the seat of it. When I saw him, he said he was extremely ill, and that he thought he could not recover. He had now pain in the shoulder-blades, and in the muscles of the arm which are attached to the trunk. He complained, also, of a pain almost allied to tic in the cheek and temple; and I also found that the pain in the spinal column had left the original site, and had attacked the dorsal vertebræ from about the fourth to the eighth. He was not so bad, however, but that he sat upright in bed, at his own instigation; and somewhat to the astonishment of himself and his attendant, I took the opportunity of giving his spine a good hearty rubbing, which shook him considerably; but observe, I had not the smallest complaint of pain to challenge my violent treatment. Now, I dare say you know the key to this case as well as I do.

But to return to hysteria. You will see abundance of hysteric hip affections, and in yet more abundance, hysteric affections of the knee. So general are these forms of disease, that I declare I think less error would result if every example of disease of these two joints, not caused by accident, which occurs in young women between sixteen and thirty years of age, were treated as hysteria. I state this opinion with the less hesitation, because we rarely have much difficulty in detecting the real disease. We can distinguish the real from the spurious much more readily than we can detect the spurious from the real. An hysteric affection of a joint is marked, first, by the sex and age of the person—generally a female, and young; second, we have more pain than in real disease; the pain is irregular—sometimes severe, sometimes entirely absent. On examination, the approach of the hand to the neighbourhood of the joint is attended by an exclamation anticipatory of pain, and a shrinking from pressure, often with tears. Pain is expressed on slight, but not increased by heavy, pressure, and rough manipulation. You may boldly shake the entire limb, while the patient's attention is engaged in some collateral thoughts. The head of the femur may be boldly revolved in its socket, and the limb carried round in all directions, at first slowly, and then with gradually increasing force.

I saw a lady, some months since, of about thirty-five years of age, who was brought up to London for surgical advice, on account of a severe affection of the hip-joint which she had suffered from, for some

two or three years. She was on the point of leaving town when I paid her a single visit. When I entered the sitting-room of her lodgings, she was lying on the sofa. I requested her removal to a bed in the room adjoining. While being carried to bed, her cries were most distressing, and some few minutes elapsed before I was admitted. When I again saw her, she said she really feared she could not submit to be examined; but I tranquillized her by speaking kindly and gently, and she at length submitted. I watched her face while I extended my hand towards her joint, and I never saw suffering more clearly depicted. I laid my hand with the utmost gentleness on her trochanter, and very gradually pressed it against the bone, and she did not shrink. Distracting her attention to the early history of her case, I quite raised her thigh, and turned it first gently, and then with force, though slightly, in all directions, and I then struck my hand repeatedly, and with force, on the trochanter. Everybody round the bed appeared astonished—and well they might be, for such force, applied to a joint that had been kept absolutely motionless for months—which appeared permanently inverted, and apparently abridged of its length, by reason of the curvature of the lumbar vertebræ—with a limb much wasted by inaction—might be deemed a curious *exposé* of its past history. This lady's health was wasted by long confinement and severe local and general depletive treatment. Of the result I know nothing, though I had one letter from the family, stating her improvement. I should mention, that the especial reason which led to my opinion being taken on this lady's case, was to ascertain whether or not her femur was dislocated, and the question was not, judging from the appearance of the limb, an inappropriate one. The thigh was drawn up, considerably inverted, and apparently fixed. All the tissues were soft and flabby, and the limb extremely pale. Any attempt to move it with the consent of the patient was followed by acute suffering. On examination, the anterior spine of the ilium was drawn up by long-continued curvature of the lumbar vertebræ, but the length of the entire limb was unchanged.

The difficulty of these cases is greatly increased by the local appearance of disease. In hip cases the position of the thigh is distorted—more or less fixed; and the whole limb is inverted. In the knee examples, the joint is swollen, and often hot. It rests, however, perfectly straight in bed. In the former we can detect the absence of that form of dislocation which its condition simulates, by the normal relation of the trochanter major to the anterior spinous process of the ilium. We are rendered suspicious of the existence of true disease by the more advanced age of the hysteric patient—by the severity, and, at the same time, the irregularity of the pain—and by the constitutional character of the patient. In the knee cases, although the joint is swollen, and the skin hot, yet the cavity of the joint is, for the most part, free from an undue quantity of fluid; and the swelling, as has been remarked by Sir Benjamin Brodie, is confined to the sides of

the ligamentum patellæ. We more generally find irregular catamenia than otherwise, although this feature is not invariable; and we have an hysteric diathesis on which to base our inquiries.

In some cases, the harmony of the muscular action of the limb is entirely lost, and one set of muscles prevails in action over its antagonists. These insubordinate agents are mostly those of flexion: the arm may be bent up to an acute angle at the elbow joint, or the fingers may be closed, or the thigh drawn forcibly and closely up on the pelvis, one example of which I have mentioned. Amputation of such limbs formerly practised, left the diseased cause behind, but to be developed in other localities of the body. All these patients betray a weak condition of bodily health; the pulse is small and quick; the tongue pale coloured; and severe and chronic cases are generally coupled with more or less of emaciation and debility of frame consequent upon it.

When local pain is a prominent symptom, it rarely fails to succumb to the external application of opium combined with a moderately stimulating liniment, such as the compound soap liniment. It is needless to apply for aid to any form of opiate tincture; nothing short of the extract will suffice: and I commonly employ the fluid extract of opium—such as is now a good deal used by the druggists in the neighbourhood in which I reside, and to several of whom I have myself introduced it.

The catamenia should, of course, be encouraged, but not forced; and, for general treatment, we must resort to iron, bark, good nutritious diet, with which a certain quantity of fermented liquor, whether beer or wine, in some form, should be combined. The profession is indebted to Sir Benjamin Brodie for an excellent practical work on this interesting subject.—*Lancet, Feb.* 24, 1855, *p.* 205.

18.—*Treatment of Sciotica.* By Dr. Peyton Blakiston, F.R.S. [Dr. Blakiston has pursued the following treatment for twenty years with considerable success. He first saw it adopted in Paris in 1833.]

A blister, about the size of a crown-piece, is placed over the chief seat of pain, which is usually the flattened part of the buttock. After it has risen well, and the cuticle has been thoroughly removed, the raw surface is sprinkled with a powder, consisting of one grain of acetate of morphia on an average, and a little white sugar. This dressing is repeated for six successive days, the surface of the blister being kept in a raw state, if requisite, by cantharides or savine cerate, or else by Albuspeyeres' plaster. This suffices for a very mild case; but, in severe cases of old standing, the pain will now be found to have left its original seat, and to have seized on the knee of the affected side. The same treatment is then applied to the ham; and, after six dressings, the pain will have generally disappeared, and the patient will rapidly recover. By this mode of treatment, eighty-three cases of

uncomplicated sciatica have been cured, without a failure having come to the knowledge of the writer. This number might have been greatly augmented had it included the results arrived at by such of his friends and former pupils as had employed it at his suggestion, and which have been no less successful than those which occurred in his practice; but he is desirous of recording such only as have come under his own immediate notice, and for the accuracy of which he consequently can hold himself responsible.

In the great majority of these cases, no other drug was administered; but, in a few, some laxative mndicine or injection was given to remove constipation.

In two or three cases, there was a tendency to double sciatica, and then the pain passed from the sciatic region first treated to that of the opposite side, and from thence down to the knee of this last side, but never attacked the knee of the side first affected.

It is right to mention, that in Hospital practice three cases were placed under the writer's care, which he considered more than doubtful, and they were therefore treated under protest, so to speak. They all turned out cases of hip disease, and therefore they are not included in those above enumerated.

The difference in the sensations felt by the patients on the first application of the morphia was remarkable; and, without any attempt to generalise, it may be stated that a close connection was observed between the sensations felt and the previous state of health. Thus the effect produced on three persons in robust health—a blacksmith, a gamekeeper, and a lady—was most intense; an extraordinary thrilling was felt over the whole body, particularly at the extremities, with great nausea, and a tendency to faint. The lady vomited incessantly for twelve hours, so that it was found advisable to reduce the quantity of morphia in the powders to half-a-grain. On the other hand, a gentleman, who had been much reduced by over-work and by long suffering, felt no effect whatever from the application of the powders, and yet he recovered in an equally short time with the others. A lady, also, who had been taking considerable doses of opium, hardly felt the application of morphia until it was increased to two grains; but this case has been excluded, because, although the sciatica was removed by the treatment, there remained an incurable disease, which eventually destroyed her.

One lady, 26 years of age, in whom the disease was not of long standing, obstinately refused to have a second powder applied; but, happily, the one application sufficed to effect a cure. In six cases the disease recurred after an interval of from five to eighteen months; and, in two of these, it recurred twice; but each attack was less severe than the one which preceded it, and yielded readily to the same treatment. It is possible, however, that relapses might have more frequently taken place without having come under the notice of the writer; but he thinks this cannot have happened very often.

Some other forms of neuralgia were also benefited by this mode of treatment. Thus, a very distressing case of neuralgia of the scalp yielded at once; and shooting pains, which frequently accompany cancer of the stomach, were sometimes much relieved by it.—*Med. Times and Gazette, Jan.* 27, 1855, p. 83.

19.—ON THE USE OF CHLOROFORM IN DELIRIUM TREMENS.

By Joliffe Tufnell, Esq., Surgeon to the City of Dublin Hospital

Regarding chloroform as I do, as a most valuable agent in the treatment of delirium tremens, as perfectly safe if given with proper precaution, and highly efficacious if used with sufficient energy, I should be very sorry to see any check put to its employment in this affection; and I shall therefore beg to detail the particulars of a case lately under my care in the City of Dublin Hospital, which may tend to confirm confidence in its use, and prove that it can be given, and given freely too, in cases which are caused by suddenly leaving off the stimulant. The case is as follows:—

J. N., aged 36, a sottish-looking individual, a tailor, was brought to the City of Dublin Hospital by his wife, at 10 a.m., on the 22nd of September last, presenting the ordinary symptoms of this disorder when fully established. The surface generally was bedewed. The eye wandering and restless; the tongue, when protruded, flabby, flocculent, and tremulous; the hands in perfect tremor when held off unsupported from the body. The pulse was full, but soft and very compressible; the conjunctivæ slightly congested. His wife stated that he had been on a drinking bout for some days, but gave it up on the 17th, apparently in consequence of sickness of the stomach having set in. He remained at home ill and unable to do anything on the 18th, and on the 19th the disease appeared, first manifested by his taking possession of a knife, which he said he must keep by him, ready to protect himself and her from a brother-in-law, who was plotting their destruction. From this time he had no sleep, and was perpetually on the watch until brought up to the hospital, a period of nearly eighty hours.

He was at once admitted, and ordered into a quiet ward, to be carefully watched, but not restrained, unless absolutely dangerous to those around him. To have a pint of porter, as he was complaining of thirst, and then a nicely broiled mutton chop, for he had not taken food for some time. Three leeches were next directed to be applied to the temple, and upon their coming off, a cold lotion to the head, after which a drachm of chloroform was to be given, and repeated at intervals at the discretion of the house surgeon, until sleep supervened.

Sept. 23rd. I visited him at 9 a.m., and received the following report:—He had taken the food and porter, and permitted the leeches to be applied, and afterwards remained quiet until four o'clock in the afternoon, when he began to be restless, and shortly afterwards vio-

lent, requiring restraint. For some time he would take no medicine, but at 6.30 was prevailed on to swallow a drachm of chloroform in a draught, and to repeat it at 7.30, again at 9.30—three drachms in three hours, but without producing any appreciable effect; and as the chloroform did not appear to be of any benefit, the house-surgeon did not push it further, but, at the same time (as I had expressed a desire that the treatment should be uncomplicated) refrained from giving any opiate. At the time of my visit the patient was tranquil, but had not slept, and there had been now no sleep for nearly 100 hours. The bowels had not been moved since admission; a turpentine injection was, therefore, directed to be given; the hair, which was very abundant, ordered to be reduced, and cold affusion to be employed for some time to the head, after which, as a brisk breeze was blowing, the bed was to be brought under the window, and the linen blind so directed as to keep a continual stream of cold air playing over the scalp. The following draught was to be taken at intervals, according to the effect produced:—

The yolk of an egg; white sugar, 1 drachm; whiskey, 4 drachms; a little nutmeg. All to be mixed well together; and to this a drachm of chloroform added at the time of being taken.

Strong beef-tea to be given in small quantities, as wished for, and chop and porter taken as on the preceding day.

Sept. 24th. At my visit at 9 a.m., I found the patient quietly asleep, and received from the house-surgeon the following statement: "Shortly after taking the chloroform he became violent, but only for a minute or two, he then tumbled off into a placid sleep, and remained so for half an hour. On awaking he was again very noisy and restive; a second dose was now given him; he became stupid for fifteen minutes, but did not sleep, and again became violent. As soon as possible the draught was repeated, and on taking it he fell off asleep. In an hour he awoke, restless and noisy, but not so violent; after a short time, a fourth dose was administered, and under it he lay quiet, but did not sleep, and he soon began to be as furious as ever. A fifth dose was now given, plain, in cold water, which made him torpid, but from which he recovered and burst out again. A sixth drachm in water was then given, soon after which he dropped off into a slumber, and remained sleeping soundly through the night, and up to the time of my visit."

At 2 p.m. I called at the hospital to see another case, and looked into the ward to ascertain how he was going on. I found him awake and quite rational; he complained a good deal of soreness of the body generally, especially of the wrists, where he had been held when violent, but made no other remark. His bowels had been freely moved

On the 26th he was convalescent, and on the 28th was discharged perfectly well.

Now here was a case, "brought on by repeated fits of intoxication, succeeded by a pause of perfect sobriety," one of deprivation of sleep

for upwards of 100 hours, and with extreme nervous excitement, yet in which chloroform was given freely and with the happiest results; and I am satisfied it may be used generally, if administered with care. By the last expression I mean having a due regard to the support of the powers of life of the individual, by food (if he will take it) and by stimulants in the form of the egg flip with whiskey as prescribed. Chloroform is a powerful sedative, and therefore certainly not to be carelessly employed, but with due caution it may be safely and advantageously used.

As regards the objection "that chloroform often fails, and is not, therefore, to be relied upon for the treatment of delirium tremens," this I believe to have been the result solely of its not having been used in sufficiently large doses, or often enough repeated. Speaking generally, I think the intervals between its administration have been too long. Half a drachm of chloroform given every fourth hour may certainly be tried at first; but, if the second dose fail, it should be increased at once to a drachm, and the periods between the doses be entirely regulated by their effects. I do not regard chloroform as a cumulative medicine; I believe that it soon passes off by the lung, as the venous and charged blood is exposed to the air cells, and with it, its effects, leaving, however, in the case of delirium tremens, a sleep of a character that cannot be likened to the suspension of consciousness, which it causes when employed as an anæsthetic. In the latter case the brain is overpowered, and insensibility from blood poisoning produced, which lasts so long as the blood is sufficiently charged with the chloroform, and no longer. In delirium tremens, on the contrary, it is not the continuous circulation of the chloroform that produces continuous sleep. The impression temporarily made upon the brain, if sufficient, gives to that organ the power of regaining a certain portion of its *vis insita*, and thence ability to discharge its proper functions.

Sleep being the suspended function, and the one nature and the system demands. it exercises or rather comes under the influence of it, and, if sufficient impression has been made by the chloroform, then the brain remains at rest until its normal tone is restored. Occasionally, it is true, we see genuine sleep follow upon inhalation of chloroform, after insensibility has been produced, and the individual, instead of returning to consciousness, passes into sleep, although repose is not needed by the brain.

Such an instance is the following:—Not long since I was sounding a young gentleman who had symptoms of calculus in the bladder. He had inhaled chloroform moderately from a handkerchief, and came sufficiently under its influence in about two minutes to have the bladder (without consciousness) carefully explored. The examination ended, the sound was withdrawn, and I accompanied his father to a corner of the room to speak upon his case. I was engaged in conversation for some time, and then prepared to leave the house, but was called back in haste by his father, who said that his son was senseless.

On returning to the room, I found the boy not as he supposed, but in a sound, comfortable sleep, from which he awoke in a natural manner on hearing his father's voice.

Now until a calmative condition is produced, the chloroform already taken may, I believe, be considered as inert, and any good effect must be anticipated only from the succeeding dose. If this too fails, and delirium and excitement continue unchecked, then, after an interval of time sufficient to warrant belief that it also has been unappreciated by the system, the dose must be repeated, and so continued, and I regard an hour as the full period that need be permitted to elapse before it is again given. Certain I am that the strength of the patient will be more rapidly expended by the continuance of the disorder—its clammy exudation, its muscular activity, and mental horrors, being left unchecked—than by any sedative influence of the chloroform.

I have no fear of its use internally. I have employed it to a degree that may startle those who have not had much experience of it. I have, in a case of traumatic tetanus in the City of Dublin Hospital, kept a young man, aged 18, under its influence for nine days and nine nights successively, during which time he took a quarter of a pint by the mouth, and a pint and a quarter were evaporated closely over the nostrils, carrying him in a state of unconsciousness through this fearful disease, and restoring him to perfect health.

I have lately, in a case of tetanic convulsion in an infant, only eight weeks old, in consultation with my colleague, Dr. Beatty, employed it even more largely still, for no less than five ounces of chloroform were expended in vapour in twenty-four hours, completely controlling the convulsions, the child dying solely from inability to swallow sufficient support.

With these facts, then, before us, and the experience of the happy effects of chloroform in treating delirium tremens, as already recorded in this and other journals, I think we should give it the fullest and fairest trial; for the "horrors" are, in a tropical climate, by no means the manageable disorder we see them here, oftentimes unyielding to opium, and frequently terminating in death; and any remedy holding out the hope of recovery, which chloroform does, should be, I maintain, most severely tested, before it is reported as *inert.*—*Dublin Hospital Gazette, Feb.* 15, 1855, *p.* 19.

ORGANS OF RESPIRATION.

20.—ON A PREVAILING FORM OF CHRONIC PNEUMONIA.

By Dr. Richard Payne Cotton, Physician to the Hospital for Consumption and Diseases of the Chest, Brompton.

[Dr. Cotton thinks that acute diseases have been numerically declining whilst chronic diseases have been increasing. Chronic pneumonia

has been generally considered to be a sequel to the acute form, but many cases would seem to be chronic from the first—original and independent diseases, running their course like any other chronic malady. The following account of this form of pneumonia is very interesting].

Commencement of the disease.—The form of chronic pneumonia to which I would direct attention, may arise in persons under the most opposite circumstances as regards temperament, age, and predisposition. The strong and robust are as often its victims as are the feeble and the fragile. The old and the young are equally liable to its attacks. I have met with cases at the ages of four and of fourscore; but the middle period of life affords the greater number of patients.

Its exciting causes are those which might induce bronchitis, pleuritis, or a more active pneumonia,—such as exposure to cold and damp, sudden changes of climatic condition, and the like. Certain states of the atmosphere, however, seem to exercise a great influence upon its development, several cases often occurring simultaneously, and having many of the characters of a true epidemic. Sometimes it is the sequel to a tedious or neglected attack of bronchitis, more especially of the capillary form of this disease, in consequence of the extension of a low, inflammatory, or congestive action into the cellular structure of the lungs.

The initiatory symptoms are not always alike, but they are for the most part slight and singularly deficient in all external manifestation of local disease. There is seldom pain in the affected lung; generally, indeed, not the least uneasiness is complained of. Dyspnœa is rarely observed; and the breathing is, in most cases, scarcely if at all affected until the pulmonary disease has made considerable progress. There is generally, however, more or less cough, which, in some instances, has a spasmodic character, and is severely distressing to the patient; yet I have seen a few cases in which this symptom was entirely absent, even at a period when part of the pulmonary structure was unmistakably impervious to air. The expectoration is no less variable; sometimes it entirely absent. I have twice seen slight hæmoptysis, and, on three or four occasions, a few brown or rust-coloured sputa; but, in the greater number of cases, and throughout the early stage of each individual case, little else is expectorated than frothy or slightly muco-purulent matter.

The pulse is at first very little affected, seldom exceeding 80 or 90 in the minute; but in this symptom much variety may reasonably be expected. Its frequency and strength, however, are of great use in prognosis, the quickness of the circulation being in constant proportion to the extent and severity of the attack. There is seldom much, and oftentimes no symptomatic fever; but night perspirations are more or less observable.

The patients are, for the most part, singularly unwilling to regard their disease in a serious light, flattering themselves that they are affected merely with dyspepsia, or with such an attack of bronchitis as a little care and a very little physic will easily remove. The early symptoms, indeed, are generally of so slight and almost negative a character, as scarcely to excite the apprehension even of the medical attendant; and nothing but their long persistence, added to a feeling of debility, with a gradual loss of spirits and of health, leads to a suspicion that the malady is after all, not quite so simple in its nature.

Progress of the disease.—The disease may thus go on for a considerable time without producing any very decided, and no alarming symptoms. Weeks, indeed, may pass away, the patient complaining perhaps of little else that general debility, together with a more or less troublesome cough or slight dyspnœa. Sooner or later, however, less flattering symptoms are too liable to present themselves. The cough becomes severe and spasmodic, breaking the patient's rest by night, and sadly harassing him by day. The dyspnœa is more marked, and unfits the sufferer for all active pursuits. Night perspirations grow more frequent and more copious. Emaciation steadily advances. Hectic fever makes its appearance. The spirits flag; and the hopeful fancies of the patient are exchanged for doubtfulness or despair.

Termination of the disease.—In by far the greater number of cases, the chronic pulmonary disease of which I have been speaking terminates, under timely and judicious treatment, favourably; the formidable symptoms just now enumerated do not occur; the patient gradually regains health and strength; and, although it may be some length of time before the disabled lung thoroughly resumes its proper functions, yet the local mischief is stayed, and finally disappears. Unhappily, however, even under the most careful treatment, the course of the disease is not always so encouraging. The cough, the night-sweats, the loss of flesh, with the consciousness of being and the necessity of acting as an invalid, tell sadly upon both mind and body, and it is only after a long, severe, and doubtful struggle that the malady is dissipated.

When the disease is destined to terminate fatally, it does so in one of three ways. 1st. By so wearing out the sufferer by its obstinacy, as to render him the ready victim to some other malady: or, 2nd. By inducing structural changes in the pulmonary tissues, which lead to a set of symptoms having all the general external characters of phthisis: or, 3rd. By laying the foundation of true pulmonary tuberculosis.

1st. Of the first of these three results I have seen but one example. This was in a patient at the Consumption Hospital, whose disease resisted, for several months, every kind of treatment, and whose health finally gave way, death taking place from an attack of diarrhœa, which, under other circumstances, would, in all probability, not have

proved fatal. In this case, the lung was simply contracted and consolidated; there was no softening of its tissue, nor any tubercular complication.

2nd. The second result is, of the three. by far the most common. After a career of a few weeks, or sometimes even of a few months, during which time the general symptoms are progressively advancing, the tissue of the lung softens, and a fatal issue is too evidently threatened. Hectic fever, with profuse expectoration, night-sweats, or diarrhœa rapidly supervene, and the patient ultimately sinks, exhausted and emaciated, as in ordinary phthisis. I have met with five examples of this kind; in all of which, had it not been for a knowledge of the previous history of the patient, and for the local disease being concentrated at the lower and middle lobes, rather than at the apices of the lungs, I might have mistaken the malady for true tubercular consumption.

Some patients, however, even at this stage of the disease, will struggle on for months, or perhaps years, the disease passing into *chronic pulmonary abscess*. When this happens, there is usually, from time to time, an expectoration, or rather a discharge of a quantity of fetid muco-purulent matter, which, for a while, relieves the urgency of the symptoms, and flatters the sufferer with hopes of recovery. But the secretion is quickly renewed, to be discharged again at a subsequent period; and the system, at length worn out, succumbs to the disease. I have lately seen this in a lady, who, for upwards of four years, has been in the habit of expectorating, little less than a pint of offensive purulent matter, every six or eight weeks. During the intervals she rallies considerably; but each attack leaves her more exhausted than the previous one, and she has, for a long time, been a confirmed invalid. Recovery, however, is not absolutely impossible, even at this advanced and apparently hopeless stage. In a few cases, the health may be restored; the lung gradually contracting, and the pulmonary symptoms disappearing. Such instances, however, are comparatively rare.

3rd. The third result does not exhibit itself, except where the disease is associated with a tuberculous predisposition. Phthisis originating in this way is invariably of a most intractable, and sometimes of a fearfully rapid kind, presenting, in many respects, the character of ACUTE CONSUMPTION. The consolidated portions of the lung seem to undergo tuberculous degeneration, and are found after death to be more or less in the condition known as *tubercular infiltration*. The patients often die before vomicæ have had time to form; but occasionally true tuberculous excavations are abundantly met with.

Pathology of the disease.—In the *first stage* the lung is, probably, in a state of engorgement, and presents the usual and well-known appearances of that condition; as a general rule, its volume is unchanged; but, in a few cases, it is, I believe, slightly augmented.

In only two instances have I been able to see the local conse-

quences of the malady. In one of these—which may be regarded as representing the disease in its *second stage*—the entire lung was diminished in volume; it crepitated slightly at the apex, but below this the whole tissue was dense, firm, and resisting, appearing, indeed, as if it had been filled up with some albuminous or fibrinous exudation, which had afterwards contracted. The pleural covering was thickened, but no adhesion existed between the visceral and costal membranes.

In the other case—which may be looked upon as an illustration of the disease in its *third stage*—the lung was softened in its structure, of a reddish-grey colour, and filled with a dark purulent matter; towards its base were several small cavities, separated from each other by soft and easily broken tissue—the lung, in fine, presented the usual appearances of *grey hepatization*.

We might, perhaps, expect in the majority of cases to meet with some evidence of pleuritic inflammation, and, probably, in none should we find that the pleural membrane had not participated in the attack. But I am anxious to draw a broad distinction between the disease I have been describing, and that which is termed *pleuro-pneumonia*, wherein the pleura is prominently, and, in most cases, principally involved. In the chronic form of pneumonia inflammation to which I would direct attention, the pleura appears to be only secondarily affected, and even then to an inconsiderable extent; there being little, if any disposition to pleuritic effusion. There may, of course, be an occasional exception; and the one form of disease may even pass into the other; but the great majority of cases which have fallen under my notice have exhibited the peculiarity in a very marked degree. It is probable, from this circumstance, that the disease has both its commencement and its intensity more frequently in the central and lower portions of the lung than towards the circumference or at its very base.

Physical Signs.—Inspection and Mensuration.—At the outset of the attack, and when the lung is simply engorged, there is no change in the form of the affected side: but, with the advance of the disease, and as the lung diminishes in volume, the thoracic parietes become more or less contracted. In cases of long standing, this contraction is usually very striking, manifesting itself even to the eye.

The respiratory movements are affected in proportion to the extent, the stage, and the duration of the pulmonary disease. When this is inconsiderable, or of recent date, it may not be easy to detect any difference in the two sides, except, perhaps, on deep inspiration; but, it is impossible for the lung to be much, or for any length of time, involved, without at once betraying its condition during the respiratory act, in the diminished movement of the costal structures.

Percussion, carefully employed, will generally, even at an early

period, indicate both the existence and the extent of the local disease. At first, there is simply diminished clearness; but, as the pulmonary consolidation increases, the sound becomes more and more dull, until, at last, it often acquires the quality which has been termed *wooden*. The sense of resistance to the percussion-stroke is also gradually developed, and is, I think, oftentimes more distinctly marked than in any other pathological condition. The *wooden* character of the sound, when fully developed, is very peculiar, and may help to distinguish the disease from pleuritic effusion.

Auscultation.—The respiratory murmur is at first either weak or harsh, or it is suppressed; it is seldom bronchial. The extent and situation of the pulmonary disease must, of course, greatly influence this particular; but weak and harsh breathing, extending for some distance beyond the dullest part of the lung, would seem to be the usual attendant of the early stage of this form of pneumonia. In some cases there is, even for weeks together, an entire absence of secretion sounds; but, more generally, the subcrepitant ronchus is more or less abundant, and oftentimes so minute as closely to resemble *fine crepitation*. The latter sound, however,—erroneously regarded as the true characteristic of pulmonary inflammation,—is very rarely indeed, to be detected. Sibilant and sonorous rhonchi are sometimes heard at different parts of the chest, and are indicative of attendant bronchitis.

There is more or less bronchophony; and the vocal and tussive fremitus are correspondingly developed.

In two fatal cases, I had an opportunity of watching the gradual changes in the auscultatory murmurs, induced by the softening process. The harsh breathing became by degrees bronchial, and subsequently cavernous; whilst the subcrepitant râles, increasing in abundance and in size, gradually gave place to the cavernous or gurgling.

Differential diagnosis.—The disease under consideration is unlikely, if not impossible, to be mistaken for any other except bronchitis or phthisis; and, to distinguish it from either of these, it is only necessary to make a physical examination of the chest.

I was consulted a few weeks back by a healthy-looking farmer, who had been gradually getting thinner, and was troubled with a dry irritable cough. He had neither dyspnœa nor thoracic pain; but his chest had never been thoroughly examined, and all his symptoms were referred to an obstinate bronchitis. On making an examination of the lower lobes of the lungs, I found that on the right side completely dull and impervious to air.

Last year I saw a gentleman supposed to be phthisical. He had long had a severe, dry, spasmodic cough; he had been getting thinner, and was much harassed by profuse night perspirations. The base of the right lung, was found, on percussion, to be completely dull and

wooden; the respiration was in that part feeble, and marked by abundant fine subcrepitant rhonchi. Here, as in the preceding case, there was no difficulty about the diagnosis, immediately that the physical signs were taken into account. It would not be easy, perhaps, to name another disease wherein the value of a physical examination is more apparent.

Treatment.—If the attack be of recent date, either cupping, or the application of leeches, according to particular circumstances, and to an extent depending upon the patient's strength, will generally be productive of marked relief. In consequence, however, of the insidious nature of the disease, it rarely comes under treatment quite at its onset; and I have found the greater number of cases far more benefited by immediate recourse to counter-irritation.

The choice between the repeated application of blisters and the continued use of an irritating liniment, must be determined by special circumstances. I have generally applied, in the first place one or two blisters, trusting afterwards to a strong liniment either of croton oil or iodine, or a combination of both. This treatment should be pursued for some weeks, indeed until some impression be made upon the pulmonary disease. More active counter-irritants, such as issues or setons, are, to say the least, undesirable; they are too exhausting in their effects, are less easily regulated in their action, and cannot so safely be discontinued when their use becomes no longer necessary.

The selection of internal remedies must, likewise, be determined by the particular stage of the disease, and by other special conditions. Where I have either discovered or suspected a tuberculous tendency in the patient, I have never given mercury; but, in the absence of this, and in proportion to the patient's strength, I have found it a valuable agent. I have generally at once administered small and repeated doses, until the gums have been slightly affected, taking care, however, not to carry it to extreme salivation. At the same time, I have prescribed some form of conium or hyoscyamus, combined generally with ipecacuanha or with squill, according to special symptoms. In a few instances, where the pulse has been rather quick, the respiration rather hurried, and the cough troublesome, I have found the addition of antimony of considerable use; but, more generally, depressing agents of this kind, even at the onset of the attack, are rather contra-indicated.

As the disease passes into a more chronic state, continued, but mild mercurial action, in conjunction with a general tonic treatment, should be perseveringly employed. The syrup of the iodide of iron,—the iodide of potassium, with some vegetable tonic,—or one of the mineral acids, with quinine or cinchona, will now become of essential service. To either of these, cod-liver oil may be advantageously united; but if I were required to say from which combination I have seen the most advantage, I should, without hesitation, name that of

the iodide of iron with cod-liver oil. But I am far from being the advocate for an exclusive, or even a general use of any such remedy; it is to the class, rather than to the individual, to which I would direct attention. When the patient is anæmic, some of the salts of iron, in conjunction with cod-liver oil are frequently useful. For children, I have often successfully prescribed a mixture of steel, wine, and cod-liver oil, twice daily; and, at night, small doses of ipecacuanha.

But it would be folly to attempt the treatment of this disease by physic only. In no other malady, perhaps, is the use of hygiene more called for, or more apparent. When the early symptoms have passed away, change of air and scene, and mental relaxation, should, as soon as possible, be resorted to. Pure country air, sea-breezes, healthful and happy associations, freedom from business and care, are now the allies of physic, without whose aid victory must always be uncertain and seldom permanent.

Even at the very earliest period of the disease, the diet should be nourishing. It is impossible to lay down very precise dietetic rules for the first stage of the attack, as everything must depend upon the activity, or otherwise, of some particular symptom, and the physical powers of the patient. Few persons, however, will bear any great reduction of their accustomed fare; and, I am convinced, that very few indeed will be found to require it.

When the disease is stationary, or passing into a more chronic stage, a liberal and nourishing diet is absolutely essential. The addition of wine or of beer is now not merely harmless, but really efficacious. To keep a patient upon a low diet because he may cough or be a little feverish would be, I am satisfied, only to treat the symptoms and to disregard the disease. The observant practitioner will, of course, occasionally see the necessity of departing temporarily from such a practice; but, as a part of the general system of treatment, he must speedily discover it vast importance.—*Med. Times and Gazette, March* 31, *p.* 308, *and April* 14, 1855, *p.* 358.

21.—PNEUMONIA TREATED BY WINE AND AMMONIA.

By Dr. E. A. Parkes, Physician to University College Hospital.

[The patient, a woman aged 24 years, was found to have "double pneumonia of a very bad kind at the base of the heart, both lungs, in fact, hepatised."]

Jan. 10. Her respirations are 78 in a minute! four times the natural amount. Her breath is fœtid; lips discoloured and cold; pulse intermittent and so quick it is impossible to count it. The woman was apparently dying. This was a case, as was well remarked by Dr. Parkes, where bleeding would have made matters worse; and yet where some

practitioners would order it. On the contrary, he ordered her brandy, carbonate of ammonia, and wine. In the first twenty-four hours she took ten ounces of wine, thirty-six grains of carbonate of ammonia, and two drachms of compound spirits of sulphuric ether. No calomel, or tartar emetic. Eighteen hours after admission it was found the pulse could not be counted, it was so quick. The respiration had fallen however, to 64. She had become drowsy.

11th. Some improvement to-day; pulse is stronger and less intermittent; respiration down to 52. She begins to have expectoration. She spits up large muco-purulent masses, to which Dr. Parkes attaches great importance. She is to go on with her remedies as before. She is in such a state of distress and excessive illness that all sitting up in the bed, and examination with the stethoscope, would be obviously improper. Percussion note dull.

13th. She is still improving. Respiration 42; pulse 128.

14th. Since yesterday the inflammatory exudation in the lungs is softening down very satisfactorily. She is much the same to-day; but the expectoration is scanty. This is the fourth day under wine and stimulants. The pulse is still coming down, as it is only 104; the respiration 38. The physical signs, which we purposely leave out, as they are unsatisfactorily obtained, are improving. Some bronchitic râles, especially about the base of the right lung, are giving way; the left lung, however, is not improving as rapidly as the right. Percussion note still ominously dull; bronchial respiration also very general. With all this manifest improvement, Dr. Parkes directed the attention of his class this day to one point still wrong—there was no increase of the natural excretions. The effete inflammatory matter in the lungs had been obviously taken up into the circulation; but the excretions were still unchanged; the skin dry and without perspiration: urine below the average; bowels rather confined than otherwise; expectoration not so plentiful as it should be. Artificial diarrhœa, he thought, was not at all of use, but helped to throw the patient back. Yet she was precisely in the position in which the old observers—Sydenham, Cullen, &c.—laid such stress on the importance of crisis in pneumonia, and where a natural diarrhœa or sweating would in all probability carry off the entire disease.

18th. Since last report, as was feared, she had fallen back. She has had no "crisis" of any good augury; but, in place of it, an attack of pleuritis. She was at one time so feverish that the question even now arose as to bleeding, and laying aside the stimulants. As there is not much effusion, Dr. Parkes, however, continued the stimulants, which are manifestly agreeing with her.

20th. Pleurisy all gone off. She is quite another woman. Pulse 100, soft and full; respiration 40; appetite improved; exudation into lungs nearly all absorbed. She now takes small doses of iodide of potassium, and is advancing towards convalescence.

Dr. Parkes draws attention to the fact, that in such cases we shall

find the lungs attacked, so to speak, unsymmetrically—one lung attacked *before* the other; and, what is of practical interest, that one lung, perhaps, will get well also long before the other. We may have a state of things which, individually, we often meet with in practice in *externe* patients, one lung in a condition over which one would place a blister, and for which we should exhibit stimulants, as ammonia, while the lung of the opposite side bespeaks an opposite treatment, leeches or antiphlogistics; one long dull and solid, the other with recent crepitus and considerable inflammation, and what Dr. Addison calls "*calor mordax.*" In the present case, and many such are met, but, we regret to say, not understood in routine practice, the constitutional disturbance first claims attention; you must disregard the local symptoms altogether for a time. It was a case, indeed, where the infinitesimal system of some quaint practitioners, joined with wine or brandy, would have effected a cure, the latter due solely to the stimulants; while all the usual depressing remedies, in six-ounce mixtures, would do harm; and the blisters and leeches of ordinary routine still further complicate the case. These, in their way, are no doubt invaluable—everything depending on the stage and character of the case in which they are used. We may have, and very often we do have, bronchitis in one lung, or in one portion of a lung, and very marked pneumonia in the other. This case will be still more unsatisfying in its treatment and ultimate result, if the practitioner is not able early with the stethoscope to detect the state of the patient.—*Lancet, Feb.* 3, 1855, *p.* 123.

22.—*Pneumonia treated by Bleeding.* By Dr. Addison, at Guy's Hospital.—[The following case shows the effect of the antiphlogistic treatment of pneumonia. This case, no doubt, (although this symptom is not mentioned in the report) would be known to be one of an inflammatory character, not only by the symptoms described, but by the absence of the *brown tongue*; which so remarkably characterizes the typhoid pneumonia of Stokes and Todd. We do not mean a yellowish tongue, but one coloured brown, or brownish, by effused and decomposed blood, which we suspect so peculiarly colours the lips, teeth, and tongue, in cases of blood poisoning. This patient was admitted into hospital with the following symptoms:]

His breathing is very much oppressed: his pulse quick, and quite of a pneumonic character; he is thirsty and feverish; his expectoration tawny or rust-coloured, viscid, and presenting that intimate mixture of mucus and blood so pathognomonic of pneumonia; he has pain in the chest, and lies on his right side. Dr. Addison ordered him to be bled to twelve ounces; the blood, as we saw it, was peculiarly cupped and buffed. It is curious, as observed at the time, that Louis believed venesection had little influence over pneumonia, other observers trusting to it almost alone; in the present case, the effect was

most beneficial. Dr. Addison also ordered him calomel and tartar-emetic every six hours, arrow-root as diet. In fact, we had here the ordinary treatment of pneumonia.

Bleeding, to be of use in pneumonia, perhaps it cannot be too often said, must be tried early, in that first state of engorgement or splenization of the lung, where, however, we unfortunately are so seldom consulted.—*Lancet, Feb.* 3, 1855, *p.* 123.

23.—*On the Absence of Chlorides from the Urine as Diagnostic of the onward Progress of Pneumonia.* By Dr. John Hughes Bennett, F.R.S.E., &c.—[In the Edinburgh Monthly Journal for April, 1854, Dr. Bennett gave an interesting article on this subject. He now relates several good cases, which confirm his own opinions, as well as those of Simon, Beale, and others. After reciting three cases, he says,]

In all these cases the absence of chlorides marked precisely the onward march of the pneumonia, whilst their presence indicated its cessation, and was generally accompanied by the returning crepitation and commencing absorption of the exudation. It still remains to be determined whether the absence of these salts is a cause or a result of exudation into the lungs—whether the interference to the respiratory function, by diminishing the amount of oxygen absorbed, gives rise to those chemical changes in the blood which react on the urinary secretion. If so, what are the nature of these changes? Indeed, a crowd of questions will be suggested to the mind of the physiologist, from the establishment of the remarkable clinical fact of which we are now speaking. That such is an important diagnostic sign I have now no doubt, and it was singularly well tested in the following case, in which there were many signs and symptoms of pneumonia, complicated with heart disease. The question on admission was whether with heart disease and bronchitis pneumonia might not be conjoined, and I was assisted in answering in the negative by the abundance of chlorides which the urine contained.

Case 4.—*Bronchitis and Pulmonary Congestion, from Morbus Cordis, resembling Pneumonia, but no absence of Chlorides in the Urine.* John Dickson, aged 44, pensioner, admitted July 21st, says that on the evening of the 19th he was seized with chilliness, followed by sweating, heat of skin, thirst, impaired appetite, and expectoration of a frothy fluid, resembling liquorice juice. He has for some time felt an uneasy feeling in the epigastrium, which, since his recent illness, has amounted to pain. Yesterday he experienced great dyspnœa and anxiety, symptoms which have continued until now. On admission there is excessive dyspnœa, with expectoration of a tenacious sputum, of a reddish-brown colour. On percussion there is no comparative dulness, but posteriorly the resonance is impaired on both sides. On auscultation anteriorly the expectoration is everywhere

much prolonged, and posteriorly there is considerable crepitation with bronchophony. Pulse 92, of good strength. The heart's sounds are entirely masked by the prolonged wheezing expiration and agitation of the chest. He cannot lie on his back or left side, is easily agitated, and frequently experiences palpitations, and cannot sleep. Abundant chlorides in the urine. Other functions normal.

℞. Sp. æther sulph. ℥ss., aq. cassiæ ℥iv.; one tablespoonful to be taken in water occasionally—to have ¼ grain of ant. tart. in solution every two hours.

July 25th. Since last report the dyspnœa has diminished, and the crepitation posteriorly continues, but the wheezing anteriorly is less. Still gelatinous sputum, specked with rusty-coloured blood. The apex of the heart cannot be felt, but a double blowing murmur is now recognizable, accompanying both the first and second sounds—the systolic loudest at the apex, and the diastolic loudest at the base. *Omit the antimony.*

July 30th. The pulmonary symptoms and signs have now greatly subsided, whilst the cardiac lesion has become more distinct. For this latter he remained in the house until the commencement of November, when he was dismissed greatly relieved.—*Monthly Journal of Medicine, Dec.* 1854, *p.* 483.

ROYAL HOSPITAL FOR DISEASES OF THE CHEST LIBRARY CHARING CROSS

24.—ON TUBERCLE.

By Dr. C. Radcliffe Hall, Physician to the Hospital for Consumption, Torquay.

[In a very interesting original article in the 'British and Foreign Medico-Chirurgical Review' for April, Dr. Hall enters into "the mode of development of tubercle in the lungs in chronic phthisis, its connexion with fatty degeneration of the epithelium, of the air vesicles, and its early manifestation by fatty epithelial cells in the sputa." He says,]

Tracing onwards from lung which has no abnormal appearance to the centre of a large crude tubercle, we find as follows:

1. The pavement epithelium of the air-vesicles is more nebulous.

2. Each epithelial cell becomes enlarged, more cloudy, more prominent when seen in profile, and is studded here and there with oil-dots.

3. The epithelial cells become still larger, and more fatty. In many of them no distinct nucleus can now be made out, but large dots of oil occupy its place. Some of the cells are detached, leaving the wall of the air-vesicle in one part bare, in another coated with compound tubercle cells. These are the preliminary stages of tubercle.

4. We now arrive at the completed tubercle, which consists of compound tubercle cells, small free nuclei in abundance, and granules; and, occasionally, in addition, of a few fatty epithelial cells in various stages of disintegration; all being held together by a tough matrix.

5. So far, the deposit has been confined to the interior of the air-vesicles. It now invades the intercellular tissue of the lungs. For the first time, we find tubercle-corpuscles amongst the fine fibres which form the frame-work of the walls and septa of the air-vesicles. These fibres are here and there seen to be broken into lengths, and the entire tissue of the affected lung has become the seat of tubercle. In the first stages, tubercle is only intra-vesicular; at last, it is both intra-vesicular and interstitial. But it is to be remarked, that distinctly cemented in amongst the pulmonic fibres, we never find any of the compound tubercle cells, but only the free nuclei and granules.

The steps of the local morbid process, then, appear to be these:—Fatty degeneration of previously normal epithelium: shedding of this; its replacement by fresh epithelium, degenerate from the first, and rapidly becoming fatty; shedding of this; its replacement by large cells containing several nuclei; shedding of these; their replacement by free nuclei and granules, embedded in a structureless matrix. Up to this stage the tubercle is intra-vesicular only. The pulmonic fibres are next enclosed and separated by the morbid exudation, and free nuclei and granules are formed between and amongst them. The tubercle is now complete.

Tuberculization of the lungs thus commences as a degeneration of a normal tissue, proceeds as a production of this tissue in a depraved form; next, as an exudation capable of following only the lowliest process of organization up to maturity. Its ulterior changes from maturity, also, are those of the degeneration of a lowly-organized product; but concerning these I have dealt elsewhere.

The structural representatives of these several stages, starting from normal epithelium, are the following, and may be considered in their natural order. They may not all be detectible at once in every mature tubercle, particularly if softening be commencing; but they are to be found in various proportions in the great majority of tubercles, provided we include along with the tubercle a certain portion of the pulmonary tissue around it.

Constant constituents of mature pulmonary tubercle at some period of its course:

1. Normal epithelial cells becoming fatty.
2. Fatty epithelial cells.
3. Many nucleated cells.
4. Free nuclei.
5. Granules.
6. Matrix.

Frequent constituents:

7. Small bloodvessels in a state of fatty degeneration.
8. Red blood-corpuscles, and orange-brown pigment.
9. Black pigment.
10. Granule cells, and glomeruli.
11. Induration matter.

[Dr. Hall enters briefly but well into each of these subjects and illustrates his paper by numerous interesting wood engravings. We would strongly recommend the reader to peruse the original article, if he wants a bird's-eye view of modern opinions on this subject, illustrated by the microscope.

The following remarks on the *Sputa*, &c., in impending, incipient, and established phthisis will interest the reader.]

Common to all.—Flat pavement epithelium, with sharp clear nucleolated nucleus, entire or partially dissolved, from mouth or pharynx. Bronchial columnar epithelium. Filmy cells, containing two or three dim granulous nuclei from tonsils. Pigment cells.

Impending Tuberculization of Lungs.—Expectoration grey, glairy, adhesive; brought up chiefly when dressing in the morning, and scarcely noticed or thought of by the patient. If innocent (which of course it generally is), it consists merely of round nebulous mucous corpuscles, in transparent adhesive mucus. If threatening, it contains, in addition, single plates, or small flakes of flat epithelium from the air-vesicles, fattily degenerating; and bronchial columnar epithelium, also presenting various degrees of fatty degeneration.

Incipient Tuberculization.—The same as the above; and, in addition, red blood-globules, more or less shrivelled and faded, enveloped in a filmy cell; a few large many-nucleated cells; granules; and frequently small casts of air-vesicles and the ultimate bronchi, in which are plainly visible epithelial cells in various sizes and various stages of fatty degeneration. So constantly have I met with enveloped blood-corpuscles in sputum which betrayed no appearance of blood to the naked eye, that I suspect that this microscopic hæmoptysis is seldom, perhaps never, altogether absent in those cases of commencing phthisis in which the more obvious expectoration of blood is wanting.

Established Phthisis.—The same as the last, largely mixed up with corpuscles of pus and mucus. Free tubercle-nuclei occasionally in small quantity.

Large Suppurating Cavities.—Absence of specific tubercle-cells, excepting portions of opaque tubercle itself happen to be expectorated. Habitually, little else than pus and mucus, and large granulous cells, unless confervoid vegetations be superadded, which is not unfrequent.

The microscopic appearances presented by the expectoration are most special and characteristic in the early stage, when other physical evidence is least marked; and may be re-enumerated as consisting of fatty epithelial cells, single or in patches, fatty bronchial cylinder epithelium, casts of air-vesicles, lined with fatty epithelium, and microscopic hæmoptysis—i.e., enveloped red blood-globules. These are enough to indicate that consumption is threatened. When the many-nucleated cells and special free nuclei of tubercle are superadded, no doubt can remain. All these I have repeatedly found in cases where the patient was not apprehensive of phthisis, but complained only of

general mal-aise or dyspepsia, as well as in other cases in which consumption was feared, but was not supposed to be yet in active existence.

Theory of the Local Part of the Disease.—The natural defensive epithelial coat of the air-vesicles is unable to maintain its own nutrition, and slowly falls into a state of fatty degeneration. It is then thrown off, and is succeeded by fresh epithelium, which is badly organized from the first, and quickly becomes degenerate and shed. This process of fatty degeneration and shedding may, perhaps, go on for some considerable time without further local disease; and we do not know but that it may happen in occasional instances, where the persons affected have their general nutrition by some means so much improved at this juncture, that good epithelium is once more laid down. This is quite conjectural; but if it ever do occur, such persons have been on the eve of becoming tuberculized, but have escaped. Ordinarily, after an uncertain number of desquamations of fatty epithelial cells, the surface of the air-vesicle forms cells which contain separate granule-bearing nuclei. In due course these also are shed and probably some may grow larger after their shedding. A variable number of crops of these compound corpuscles may succeed each other, according as the wall of the air-vesicle maintains, for a longer or shorter period, sufficient organizing power to make them. The wall eventually becomes bare in places, and the albuminous part of the *liquor sanguinis* has no longer anything to hinder it from escaping into the air-vesicle. It does so escape; coagulates and corpusculates into such small, compact, and for the most part non-nucleated, cells, as its feeble organic force permits; and these are the free nuclei, or tubercle-corpuscles proper. That portion of excluded plasma which is unable to perform even this feeble act of development, falls into amorphus granules. The adjoining air-vesicles, under a continuance of this process, are crammed with desquamated cells, and with tuberculous exudation. By the resultant pressure, the remnant of vitality which the enclosed intercellular pulmonary tissue (viz., the septa and walls of the air-vesicles) retained, is still more impaired; exudation infiltrates amongst the pulmonic fibres, and corpusculates in their interstices into the free nuclei, but not into the many-nucleated cells. The tubercle is now complete. Up to this time it may have been unattended by inflammation, or inflammation may have attended its course throughout.

The relation of inflammation to tubercle is far too important to be cursorily dismissed, and is discussed at length elsewhere. I will merely state dogmatically here that tubercle *may* pass through all its stages *except the destructive ones*, without the coincidence of anything which we can justly term inflammation. But on the other hand, it seldom does. Tubercle may commence with inflammation, or be quickly followed by it. Inflammation, in short, may step in at any stage, and almost invariably does so at some stage, and is a necessary antece-

dent to the fatal issue of softened tubercle. Inflammation practically, therefore, exercises the greatest influence upon the course and termination of tubercle.

If we choose to say that when pulmonary tubercle commences with pneumonia it has an inflammatory origin, there is no refutation at hand. But as tubercle is not *always* of inflammatory origin, it cannot be essentially an inflammation. It is rather always a degeneration, which has a very habitual relationship to inflammation as a concomitant.

The exudation poured out by inflamed blood into inflamed tissue either fibrillates into induration-matter, or corpusculates into cells, for the most part round and nucleated, which becomes ultimately organized into tissue, or dissolved and absorbed, or changed into pus cells, according to circumstances ; or else, the inflammatory blastema dies rapidly and at once. Tuberculous blastema does none of these after precisely the same type. And when tuberculous exudation is attended with inflammation, we find side by side the normal products of inflamed blood, and the specific products of tuberculous blood. The two are not mutually convertible. It follows that either that part of the blood-plasma which forms tubercle *cannot* undergo the changes of inflammation ; or else, that when some of the blood-plasma is inflamed another portion escapes the process, and is exuded in company with the first, to pursue afterwards a distinct course. In either view, the distinction between tubercle and all the known products of inflammation is obvious. But it may be said, that tuberculous inflammation may be of a specific kind, leading to the specific product of tubercle. Not so : for we find the inflammation which attends tubercle is just like common inflammation in its course, and in its products. If, again, it be urged that part of the process may be common inflammation, and part specific inflammation, proceeding side by side in the same portion of tissue, this seems to be almost a *reductio ad absurdum*, since upon such terms every known disease might be called an inflammation.

Natural Alliances of Pulmonary Tubercle.—Viewed in its twofold aspect, as primarily a degeneration of existing structure, and secondarily an exudation which corpusculates into very lowly-organized cells, tuberculization of the lungs has a generic relation with similar morbid affections of many other organs. And such allied affection of any other viscus, when it happens to coincide with pulmonary tubercle, may be looked upon as equally con-generic, whether it stop short at the stage of fatty degeneration, or pass on to that of tubercular formation. Hence the frequent association of phthisis with fatty liver and fatty heart, and its occasional association with the fatty variety of Bright's disease. The occasional occurrence of this affection of the kidney, the more rare occurrence of apoplexy from fatty degeneration of the small bloodvessels of the brain in persons of phthisical habit, but who are not the subjects of developed phthisis ; and also the antecedence of general fatness of integument, the fat being less firm than that which indicates mere excess of good

nutrition, in some persons who afterwards become phthisical, may point in the same direction. And several of the not unusual complications of phthisis, as well as some of its customary features, are possibly due to a similar kind of degenerative tendency. Thus, the thinning of the coats of the stomach which occasions the enlargement of that viscus in phthisis, though chiefly owing to simple atrophy of the muscular coats, may be in part also due to fatty degeneration. The hooked and clubbed nails, to fatty degeneration with enlargement of one portion of the bed of the nail. Scrofulous lymphatic glands, to enlargement, fatty degeneration, and accumulation of lymph-cells; or to positive tubercle; or to both, in different instances; inflammation being either coincident or absent. In like manner, the falling off of the hair, so common in females who are phthisical (as well as in many others whose mal-nutrition arises from more innocent causes), may greatly depend on fatty degeneration of the cells secreted by the hair-bulbs unfitting them for development after the existing hair is shed.

Upon these latter points (with the exception of the lymphatic glands), I have no certain knowledge; but reverting to the three instances in which the alliance is clear and sufficiently common—viz., fatty degeneration of heart, liver, and kidney, there is more to be said.

Muscle is not liable to tubercle. We have, therefore, no difficulty in admitting that fatty atrophy of the heart, when it occurs in phthisis, may be directly related to the tuberculous disease, although tubercle itself is not found in the organ. The degree in which the liver and the kidney are prone to become the seat of tubercle is not quite established. All that can be affirmed is, that whilst they are occasionally tuberculized, they are much more liable to be merely affected with fatty degeneration. When so affected in conjunction with tuberculization of the lungs, we need not hesitate to admit a direct relationship between the several organs diseased, in regard to the very essence of the disease. In the fatty form of Bright's disease, the similarity to the primary steps of chronic tuberculization of the lungs is particularly close, keeping in view the normal peculiarities of the two great excretory organs involved.

The lung is a chimney, which requires mere ventilation for carrying out its vaporous excretions. The kidney is a main sewer, always draining and periodically flushed. The internal surface of the lung keeps in all the blood except that portion which evaporates during its passage through the pulmonic capillaries. When, therefore, this surface has lost its natural defence, it does not instantly give exit to complete blood-plasma in excretable quantity. If it does so, there is often a large pouring forth, the blood escapes entire, red globules as well as plasma, and we have hæmoptysis. But in the kidney the internal surface is normally accustomed to let out the watery part of the blood, and so, when bared of epithelium in places, it readily gives passage to

a portion of fluid plasma, with or without some red corpuscles, and we have albuminous urine, with or without hæmaturia. In the lung, tubercle follows; as a rule, it does not in the kidney, perhaps for these reasons:—So long as the plasma is periodically washed away with the urine, there is no material to corpusculate left behind; and when checked elimination of urine at length ensues, its consequences generally put an end to life too speedily to permit the question to be tested, not to mention the altered blood-crasis then induced. Moreover, when tubercle *is* laid down in the kidney, its seat of election is the cortical parenchyma, and not the interior of the uriniferous tubes. And although the exact relation of tuberculous deposit to the minute structure of the cortex is not clearly ascertained at present, it is quite possible that the free discharge of albumen from one portion of the gland may oppose the formation of tubercle in the other.

As in chronic phthisis the earliest local process consists in the enlargement and fatty degeneration of epithelial cells, and more or less accumulation of these in the air-vesicles; so also, in some cases in which, had the patient lived, he would probably have shown the customary signs of the fatty variety of morbus Brightii, do we find the uriniferous tubes so choked up with detached granulous and fatty epithelium, that the urine only filtrates through, and carries no albumen with it. Such a condition we shall generally find after death, when, in the last stage of slow phthisis, there is œdema of the lower extremities (irremovable by remedies), without any albumen in the urine, but with a copious sediment of phosphates.

In practice we notice, in respect to this subject, three kinds of case: the one just mentioned; phthisis with albuminous urine; and phthisis arrested, in which the patient dies at some future time of Bright's disease.

Is, then, in conclusion, tuberculization merely an advanced stage of fatty degeneration when this affects certain textures? Certainly not, since fatty degeneration is related to many other morbid conditions besides tubercle, and having little in common with it. But it may be, that it is a constant attendant upon tubercle; in which case we have a right to consider tuberculization as fatty degeneration *plus*—something else. If so, all that is known respecting fatty degeneration has a certain reference to the etiology and treatment of phthisis.

Not to occupy space by compiling information easily obtained from the writings of Mr. Bowman, Dr. Quain, Mr. Paget, and Dr. Handfield Jones, I will merely state here that I have succeeded on a large scale in inducing fatty degeneration of various viscera by keeping animals under such conditions as entailed mere inactive discharge of the functions of these viscera.

Neither is tubercle in the lungs the necessary consequence of shedding of the epithelial cells in the air-vesicles, and of subsequent exudation of plasma; for this occurs to some extent in simple pneumonia, and to a much larger extent in the intensely-congested lungs after

section or ligature of both paria vaga at the same time. Here the deep, forcible, and very slow and infrequent inspirations, and the congestion of the pulmonic bloodvessels, acting as pressure alternating in opposite directions, cause the serum to transude and wash off the epithelium of the air-vesicles. The serum is churned into a spumous froth; hemorrhage follows in a degree which varies from mere reddening of the foam to pulmonary apoplexy. Inflammation and suppuration may afterwards ensue, but no tubercle. As the animal seldom lives long, how can we know this? If a few days be allowed to intervene between the injury of the two nerves, the animal will sometimes live. Birds will generally survive under any circumstances. Killed some months afterwards, we are able to trace in these animals the morbid actions which ensued in the lungs, and we find no trace of tubercle.

Neither fatty degeneration alone, nor exuviation of epithelium alone, nor mere exudation alone, is enough to constitute tubercle. The exudation must ever be at the first one of tuberculous blood-plasma.

But, in point of fact, as regards the lungs, are these three morbid processes ever conjoined in sequence, unless the blood be tuberculous? Does it ever happen that the epithelium of the air-vesicles degenerates fattily in considerable quantity, and is shed; and that this is succeeded by exudation into the air-vesicles, except in cases of tubercular phthisis? To this question, I am not at present prepared to furnish a positive reply.—*Brit. & For. Med.-Chir. Review, April*, 1855, *p.* 479.

25.—*Remedies in Phthisis.*—[In a visit to the Brompton Hospital the writer notices some of the remedies which are there used. He says:]

Among a few of the medical novelties we noticed, and which are found useful at Brompton, are divers forms of inhalations: one an inhalation of hydrocyanic acid vapours, of course in small quantities; inhalations of chlorine and inhalations of the chemical substance chloroformyl, inhalations also found very useful of conium and creasote.

A mixture of tannin, nitric acid, and lupulin is very valuable in the *night sweats* of the worse cases; the oxide of zinc also is found to answer wonderfully in some cases which will even resist acids and tannin; as practical facts not generally known, these valuable results cannot be too generally made public.

A mixture of decoct. hæmatoxyli and aqua calcis, not very chemical but very valuable, is used in *diarrhœa*. A solution of iodine as a caustic, now general in hospitals, is also found most useful for painting the chest; it is simply one part each of iodine and the iodide of potassium and two of rectified spirit. Dr. Quain also finds a solution of nitrate of silver, to act almost "like a charm," when applied in some cases with a probang to the top of the larynx and to the vocal cords.—*Med. Circular, July* 5, 1854, p. 5.

26.—*Treatment of Irritable Stomach in Phthisis.*—Several very pleasing cases, illustrative of the good effects of hydrocyanic acid and bismuth in the treatment of irritable stomach in phthisis have recently been under observation among the out-patients at the City of London Hospital for Diseases of the Chest. The patients had been long subject to nausea and attacks of vomiting, and were quite unable to retain cod-liver oil on the stomach, very often rejecting also ordinary food. The mixture prescribed consisted of 3 minims of the hydrocyanic acid and 10 grains of the trisnitrate of bismuth made into a draught with mucilage and green mint-water, and taken thrice daily. By its aid several patients have got quite rid of their troublesome stomach symptoms, and are now taking the oil easily and with benefit. The class of cases best suited for this treatment are those in which the tongue is generally clean, and in which the disease is undue irritability rather than disordered function. The cases alluded to have been under the care of Dr. Peacock and Dr. Risdon Bennett.—*Med. Times and Gazette, Dec.* 16, 1854, p. 614.

27.—*Laryngitis treated with Nitrate of Silver.*—Dr. Ebert (*Annalen der Berlin Char. Krankheiten*, 1854,) employs inhalations of nitrate of silver in substance, with great benefit, in all inflammations of the laryngeal mucous membrane. He has employed the nitrate of silver also in solution after the manner of Green, but has never been able to satisfy himself that the larynx was really entered. The mode in which the solid caustic is introduced is as follows:—Three grains of the nitrate are mixed with one drachm of sugar; the powder is placed in a steel pen, which is itself firmly inserted in a quill open at both ends, the little apparatus is then put into the mouth, so that the end of the steel pen shall be on the root of the tongue, then the lips are closed round the quill, and the patient respires forcibly. The first attempt is almost always a failure, and the nitrate is only tasted on the root of the tongue; but the patient soon learns to manage it very well; a little cough and irritation follow, but no uneasiness. For young children this method does not answer, and a spinal apparatus must be used.—*Med. Circular.*—*Dub. Hospital Gazette, Nov.* 15, 1854, *p.* 314.

28.—*On the Use of Iodine in Phthisis.*—The following remarks occur in a paper by Dr. Barnston upon the Parisian hospitals, contained in the 'Montreal Monthly Journal of Medicine and Surgery;' it refers to the practice of M. Piorry in phthisis:—"A patient arrives in the ward; he is introduced to M. Piorry, who immediately subjects him to a lengthened examination, in order to ascertain the physical qualities of every organ in his body. Having satisfied himself that

there is a circumscribed induration in the upper part of both lungs, this is carefully marked out with a pencil on a piece of canvas, with various shades, indicating the amount of condensation. A plan of treatment, sanctioned by long experience as most trustworthy, is now adopted, and in such cases as the present, where the consolidation is assumed to be tubercular, iodine is the all-powerful remedy. It is accordingly administered in every possible way: for example, it is given—1st. Internally in the form of solution, and if the stomach is not well able to bear it, after some time, iodine enemata are substituted. 2nd. In the form of vapour, by inhalation, which is resorted to once to three times in the day. 3rd. In form of ointment, by friction upon the cutaneous surface adjacent to the diseased parts. 4th. By means of iodine baths; and lastly, the whole atmosphere is impregnated with the atoms of iodine, by which a constant supply is furnished to act directly upon the tubercular deposition. The patient is thus subjected to treatment, and every successive examination, made with the same minuteness and precision, at intervals of two or three days, shows a slight, but yet distinct and satisfactory diminution in the circumference of the induration, till at length the lapse of time intimates a complete disappearance of the dulness, and return of the lung to its normal condition.—*Dub. Hosp. Gazette, Nov.* 15, 1854, *p.* 314.

29.—EMPYEMA TREATED BY INJECTION OF IODINE.

By John Windsor, Esq., Senior Surgeon to the Manchester Eye Hospital.

[Mr. Windsor being satisfied in this case that fluid existed in the chest, passed a grooved exploring needle along the upper surface of the seventh rib. About 52 ounces of puriform fluid were thus withdrawn, and plaster and bandage were then applied. In about six weeks, however, the man relapsed for the third time.]

On this day (May 30th) I operated for the third time, and drew off from near the same part of the chest two pints and a half, or forty ounces, of sero-purulent fluid, but certainly less puriform than on the two former occasions. Immediately afterwards I injected five ounces of the following solution:

℞. Iodinii ℥i.; potassii iodidii ℥ij.; spiritus rectificati ℥j.; aquæ puræ ℥v. M.

After retaining it in the chest a short time, and moving him a little about to bring the injection better into contact with the pleural membrane, I withdrew my thumb from the canula, when about an ounce of the injection returned through it, the rest remained in the cavity. On this occasion I was assisted by my son and two pupils; and another gentleman was also present.

He bore the operation again very well, but a troublesome dry shaking cough almost immediately succeeded, as after the first thoracentesis;

it was, however, again soon relieved by taking a little warm tea, and he remained pretty easy the rest of the day.

May 31st. The pulse was 96; the tongue was whitish, and he complained of an unpleasant taste or sensation about his mouth and throat; his appetite was indifferent; the cough was troublesome. He had not much sleep last night. The bowels had not been moved since the previous morning. He complained of more uneasiness in the chest, and of a sort of general soreness over the left side, probably referrible to the action of the iodine injection. He lay at present most easily on his back, or slightly towards the left side. A large quantity of urine, about four or five pints, was passed in the afternoon after the operation, as happened somewhat similarly after the two former tappings. He was directed to continue the mixture, and occasionally the pills.

June 1st. He felt better this morning. The pulse was 84, the soreness of the chest was going off, and he could lie on either side, but neither his sleep nor his appetite were yet good.

June 7th. He now had begun to go out of doors, and walked a good deal daily, of which exercise he was very fond. The pulse was 92; the bowels were regular; the unpleasant taste of the mouth was gone; the appetite and sleep were better; the cough still continued, with occasionally a little mucous expectoration.

As stated above, he had a disagreeable taste in his mouth for some days after the operation, probably from the presence of iodine in the secretions. It may be added, also, that on testing the urine on the third day after the operation with liquid starch and sulphuric acid, it still afforded a distinct violet tinge.

Auscultation and percussion indicated nothing new, except that there was a very resonant space between the left nipple and the sternum.

June 15th. Since last report he had continued going on well. He now lay quite as easily on the right side as on the left side; he walked out a good deal, almost daily. He had still some cough, for which he was prescribed a linctus, with morphia murias; and was ordered to continue his former medicine.

June 20th. His cough was now less frequent and easier, with a little mucous expectoration, and his breathing was pretty easy, under exercise out of doors; he had no pain, and scarcely any feeling of soreness of the left side, and he could lie in any position. The feeling of "swilking," as he called it, or gravitation of fluid from one side to the other, which he used to experience in turning in bed, had now gone off.

July 1st. He said that he now felt free from complaint, having no cough, pain, or soreness of the chest. He purposed going into Cheshire for about a fortnight.

On examining the chest, I found the heart beating now most distinctly nearly in its natural situation, or just to the right of the left

nipple. On desiring him to breathe rather deeply, the left lung seemed to admit air nearly throughout, although the respiratory sound was not so loud as on the other side. The resonance on percussion now also approximated to that on the right, but the percussion was dullish in the situation of the heart, where it had lately been resonant. The intercostal spaces had become about as distinct as on the other side.

July 20th. He called upon me after his return from the country, and said that, feeling well, he was about resuming his occupation.

Sept. 9th. I saw him again, and found him pretty well, and pursuing his occupation. There was yet little, if any, depression or contraction of the affected side; and he indeed complained of nothing, except a little weakness or aching about his shoulders occasionally, probably ascribable, at least in part, to the laborious nature of his employment.

Remarks.—In the preceding case, which seems to have terminated so satisfactorily, I was induced on the third time of tapping him to add the use of an iodine injection, in consequence of reading a paper (pointed out to me by my son) published by Dr. F. A. Aran, physician to the La Pitié at Paris, in the 'Union Médicale' (Nos. 97, 98, and 99), 1853, and also inserted in the 'Encyclographie des Sciences Médicales for October 1853. Dr. Aran there recapitulates in part what has been done, especially of late times, in regard to injection of the pleural cavity after thoracentesis in cases of effusion, in consequence of acute or chronic pleurisy. In such cases, as Dr. Aran remarks, patients often sink under exhausting hectic fever, as in phthisis pulmonalis; or, from the formation of a pleuro-bronchial fistula, suffocation may occur by too great an accumulation of fluid in the bronchial passages; or sometimes, especially, perhaps if air be admitted, from the fluid becoming vitiated, and thus poisoning, as it were, the blood. In some of these cases of fluid, generally sero-purulent accumulation in the pleural cavity, it finds an exit by a sort of abscess forming in the parietes of the chest, as occurred in the case of Morris, published in my former paper in the fifth volume of the 'Transactions' of this Association. Thus the pleural effusion may, when not re-absorbed spontaneously, be sometimes discharged by communication with the corresponding lung and bronchial tubes; at others, by a communication with the external parietes of the thorax; or, indeed, by a combination of both these modes. Of this last form, apparently, I attended a case this year in consultation with Mr. Robb. It occurred in a Mrs. Kitchen, Chester Road, Hulme, aged about 60, and the mother of a large family. Mr. Robb had attended her a considerable time before I saw her. Besides expectorating a good deal of puriform fluid, occasionally tinged with blood, subsequent to an attack of pleuro-pneumonia probably of the right side, two abscesses formed soon after I saw her, on the lower part of the right side, one a few weeks after the other. They were both opened, and, especially the first one, gave

exit to a considerable quantity of pus, increased during expiration and coughing. She became gradually exhausted, and did not long survive the formation of the second abscess. Unfortunately a *post mortem* examination could not be obtained. In this case there was great dulness on percussion over a considerable portion of the right side, and only slight respiratory murmur could be heard anteriorly at the upper part of the chest, in the subclavicular space, and posteriorly near the spine.

Dr. Aran cites two cases of thoracentesis for *acute* (purulent) pleurisy by Dr. Trousseau, but both terminated fatally at last, which was not remarkable, as they were complicated with other serious affections. Dr. Trousseau had also operated four times for *chronic* purulent pleurisies, but all terminated fatally at no distant period.

From the pyogenic disposition of the pleural membrane, and therefore the great disposition to relapse after thoracentesis, it becomes a question, and a very momentous one in preventing a fatal termination, so common in this disease, how far we may be able to counteract a recurrence of the effusion by the use of injections, and most effectively perhaps by the use of iodine ones. M. Boinet has referred to three cases where they have been employed, once by himself, once by M. Boudant, and once by M. Massiani. In the last case a cure was effected in three months, but with considerable deformity of the chest ensuing, along with depression of the corresponding shoulder, and lateral inclination of the vertebral column. It is well known since the researches of Laennec, that this change may follow a natural cure, and was indeed referred to in my former paper, as occurring in Mr. Burton's patient, Mr. Liversedge, the artist. In M. Boudant's case, a cure was effected in six months, but with some sinking of the ribs, and slight deviation of the sternum. In M. Boinet's case, a cure was effected in four months, but with flattening also of the ribs. In all these three cases air was admitted into the chest, which probably modified injuriously the character of the fluid. If this admission of air be prevented, either totally or almost so, by making a valvular opening, as I did in each of the three operations in the case related above, a speedier cure may be expected, with less subsequent contraction of the chest, probably from the readier adhesion of the opposing membranes, and consequent obliteration of the pleural cavity.

In two cases operated on by Dr. Aran also in this way, one of acute and the other of chronic pleurisy, the former terminated fatally by the supervention of subacute peritonitis; but the latter, occurring in a locksmith, aged 24, and of fifteen to eighteen months' duration previous to treatment, terminated successfully after a single iodine injection.

When we consider the result of the treatment of these six cases of pleuritic effusion by iodine injection, five out of the six being successful, and that the other would not improbably have been so also, had it not become complicated with another affection,—it must, I think, be conceded that it is one offering considerable advantages.

I am not aware of any other case besides the one related in this paper having been yet treated similarly in this country; but from the satisfactory result in this one instance, conjoined to those of a few similarly treated abroad, I think we are encouraged to give it a further trial in cases apparently adapted for it. Such cases seem to be those of chronic uncomplicated pleuritic effusion. Similar success could scarcely be expected when this empyema is the result of a vomica or vomicæ bursting into the pleural cavity, or where the effused fluid has been suffered to remain so long as to form external communication through the parietes of the chest, or where both these states are coexistent. Of all these forms and complications, I doubt not, most have seen examples; and instances of them are referred to both in this and in the former paper. However, even in such circumstances, where a permanent cure is unattainable. great temporary relief to the oppression and dyspnœa is often afforded by the operation of thoracentesis. The process itself of withdrawing the fluid by the use of the trocar and canula alone, without any previous incision or dissection, is one of extreme simplicity; and, though I have now performed it many times, no unpleasant accident has ever occurred.—*Association Med. Journal, Oct.* 20, 1854, *p.* 944.

30.—*Iodine Injection in Pleuritic Effusion.* By Dr. Atlee. —Dr. Atlee relates the case of a gentleman (age not given) from whose side nine pints of pus were discharged on the 16th December, through a trocar passed into the left side of the chest between the seventh and eighth ribs, midway between the spine and the sternum. No further discharge took place until the 2nd of January, but the closed wound then reopened, and from that time about a pint of pus continued to be discharged daily, with the effect, in spite of restoratives, of rapidly wasting the patient's strength. On February 2nd, ʒ½ of liq. iodin. c. (U. S. Disp.) diluted with ʒi. of tepid water, was thrown in without inducing any ill effect, no precaution against the admission of air being taken; and next day the amount of discharge was diminished by one-half. Each day the strength of the injection was increased, and on the 7th February the iodine was used undiluted. The amount of matter discharged rapidly decreased, and by the end of February the flow had ceased, the patient daily recovering strength, and in a few weeks later being able to go to business. When seen three years later he was quite well, and had insured his life. On examination the left side of the chest was found measuring 2½ inches less round than the right. The respiration was normal, and the heart's action was audible in the natural locality.

During the discussion upon this paper, Dr. Bowditch related an interesting case which occurred in the person of a young woman, who, during a three months' treatment, had her chest tapped seven times. A steady improvement and a diminution in the size of the collection

ensuing each time, and the intervals between the operations being gradually increased. During all this time pregnancy steadily advanced, and the fœtus was vigorous in its movements. At first a large trocar was employed, so that by leaving the canula *in situ* a fistulous opening might be produced; but it became displaced, and caused much more irritation than the small one employed on subsequent occasions. Dr. Bowditch observed that during the past three years he has operated with the small trocar seventy-seven times, and in no instance with ill effect. In all the cases when fluid was obtained relief followed; and in some the operation was the chief or sole cause of cure.—*Amer. Journal Med. Science.—Med. Times and Gazette, April* 14, 1855, *p.* 371.

31—*Treatment of Catarrh.*—[Dr Lombard, of Geneva, suggests that catarrh should be treated as follows:]—A piece of metal is heated in a spirit lamp, and a few grains of powdered opium having been sprinkled upon it, the patient is directed to hold his head in the fumes, and to make a few forced inhalations. It is said to afford most marvellous and speedy relief to the distressing pain and sense of weight so commonly felt in the frontal sinuses. As addressed to this particular symptom, this expedient might be advantageously combined with "orange-juice treatment," so useful in allaying the fever, restlessness, and general disturbance which often attend this common and most disagreeable complaint.—*Med. Times and Gazette, March* 31, 1855, *p.* 310.

32.—ON COD-LIVER OIL.

By Dr. E. Headlam Greenhow, Physician to the Blenheim Street Dispensary.

Of the several kinds of cod-liver oil in use, the pale is that which is best borne by the stomach, and, on account of its comparative freeness from odour and taste, least objected to by patients. In my experience, the darker oils much more frequently nauseate, and almost invariably, after a time, induce gastric disturbance of such a character as obliges the discontinuance of the remedy. A few years ago, I very often found the oil to disagree, and was in consequence induced carefully to examine the several kinds used by my patients. Of these, many were either not cod-liver oil at all, or this was mixed with seal or some other oil of a cheap kind. Sulphuric acid is a satisfactory test of the genuineness and purity of cod-liver oil. A small quantity of the oil to be examined being placed upon a white tile, or a piece of clean writing paper, two or three drops of sulphuric acid on being dropped into the centre instantaneously occasion a number of radiating currents of a delicate and beautiful lilac hue; the genuineness

and purity of the oil is in exact relation to the richness and persistency of this colour, and the rapidity with which it is produced. In very rich oils the lilac colour, after a few seconds, occupies nearly the whole disc of oil, excepting the small portion directly in contact with the acid, which, although it displays the same tinge for an instant, very speedily turns of a deep purple-brown from the charring of the oil. A careful trial of seal oil, train oil, sperm oil, and the several vegetable oils in common use with this test has in no instance given a reaction which could be mistaken for that of cod-liver oil, the colour in these cases being entirely free from purple or violet tinge, and varying in the several kinds from ash to very dark brown. Skate-liver oil, for a specimen of which I am indebted to my friend Dr. Hassall, cannot readily be mistaken for cod-liver oil, its colour being more yellow, approaching to a very bright amber; its reaction with sulphuric acid very nearly resembles that of some inferior kinds of cod-liver oil, but is rather of a reddish-purple than lilac colour, and the effect is more confined to the portion of the disc immediately touched by the acid. Ray-liver oil, which is analogous to skate-liver oil, is used in Germany indiscriminately with cod-liver oil; and although the specimen of skate-liver oil I have examined is less pure, and, from being thicker, more nauseous than cod-liver oil, it is possible that if manufactured with the same care it would prove equally efficacious.

Several years having elapsed since these experiments, I have carefully repeated them for the purpose of this paper. I have also tested the value of sulphuric acid as a test for cod-liver oil, by endeavouring to manufacture a spurious oil from pure seal oil, combined in various ways with iodine, iodide of potassium, and ox-gall, both separately and in various degrees of combination. Specimens of cod-liver oil, manufactured in London, in Newfoundland, and imported from abroad, have likewise been carefully tested, with a view of ascertaining their comporative value. These experiments have been repeated many times with great care, and as the quantity of oil and acid in each was as nearly the same as possible, the result may be considered as satisfactory. As the subject is possessed of considerable interest, I shall briefly state the nature of the trial and the result in several of these experiments, premising that each was tried repeatedly, both on the same and different days, and that notes of the result were taken on the instant.

1. The first trial was with cod-liver oil itself, two specimens of which, roughly disintegrated, on being submitted to the test, afforded the peculiar lilac reaction of cod-liver oil, but, as might be expected, in a less intense manner. One of these specimens had been more than twenty-four hours out of the fish prior to its examination; and as, on a microscopic investigation, there was no trace of granular or secreting cells, the entire substance appearing to consist of fibrous tissue containing innumerable globules and droplets of oil of various sizes profusely distributed throughout it, and possessing the brilliancy

and regularity of milk globules, which they closely resembled; a second liver was procured from a fish immediately after death, and submitted to examination within three hours. This gave the same reaction with sulphuric acid as the former one, but required a more perfect disintegration. On examination with the microscopie, the secreting cells were found to be distended with oil as to cause a slight irregularity in their outline; the cell walls were extremely delicate, and yielded their contents freely on the earliest approach of putrefaction, to which the liver is peculiarly prone. The fresh liver is extremely pale-coloured, and tolerably firm, and the oil, which it contains in great abundance at this season of the year, appears perfectly pellucid and pale-coloured. After the fish has been a short time dead, the liver loses its firmness, and such changes take place in its delicate structure as materially to alter its appearance under the microscope.

2. Seal oil, sperm oil, whale oil, and cod-liver oil, were each carefully tested with sulphuric acid, the result being that the other oils gave a brown reaction varying in the different kinds, and not to be mistaken for the peculiar lilac colour afforded by cod-liver oil.

3. Eight specimens of cod-liver oil procured from different druggists, were tested simultaneously. All reacted characteristically, but there was a material difference in the intensity and richness of the colour. In some the colour appeared instantaneously, in others more slowly. The duration of the lilac hue, before passing into brown, varied materially, and, for the most part bore a direct ratio to the richness of the colour which was most persistent in the best oils.

4. Two samples of cod-liver oil prepared in Newfoundland, having previously been tested and found to be genuine, were exposed to the acid at the same instant with four samples manufactured in London, with the result that, with one exception, the reaction with the Newfoundland oil was richer than with the English. The excepted oil was as perfectly as possible freed from the stearine, of which the Newfoundland oil is said to contain a smaller proportion.

5. A specimen of brown cod-liver oil was submitted to a comparative examination with two London and one Newfoundland made samples, the result being that the richness of colouring in each was nearly alike; but that, whilst the Newfoundland and English pale oils gave the characteristic lilac reaction, that of the brown oil was much redder and less characteristic. This experiment, varied in several ways, and with several specimens, both of brown and pale oil, was repeated at least forty times, and on several days, with the like result. The brown oil is less fluid than pale oil, in this respect more nearly resembling seal oil. The same is the case with the skate-liver oil in my possession, although it is much purer looking, and more fluid than several specimens of the brown oil that I have examined.

6. Small quantities of the best Newfoundland and London oil were carefully heated for from ten to fifteen minutes in a test-tube held over a spirit-lamp. Copious bubbles of air were at first given off, and

the lamp removed as soon as the oil began to give off vapour. The result was slight thickening of the oil, without any diminution of the transparency; the oil also became darker in colour, so as closely to resemble the best kinds of brown oil. On being tested comparatively with the same oil unheated, and with the brown oil, the reaction was found to be much less characteristic than the former, and to have much of the redder hue of the latter.

7. Skate-liver oil, of a rich amber hue, was exposed to the same comparative test with Newfoundland, London, and brown oil. The colour yielded by the skate oil more nearly resembled that afforded by the brown oil than the others, partaking more of a red than lilac tint, but was intermediate between the two kinds.

8. Two specimens of oil, the one divested of stearine, the other in its natural condition, were tested with this result—that the former reacted more rapidly with the acid, but both were equally rich in the characteristic hue. That which retained the stearine lost the lilac colour earlier than the other.

9. Specimens of stearine from cod-liver oil, procured from two different manufacturers, were tested, and found to possess the characteristic reaction of the oil itself, which speedily turned to a dirty-brownish hue.

10. Equal quantities of pure seal oil and of pale English cod-liver oil were carefully mixed, and then tested; the reaction was less intense, and the lilac very soon passed into a dirty-looking colour.

11. The same experiment repeated with Newfoundland cod-liver oil gave nearly the like result, the reaction at the first more nearly resembling that of pure cod-liver oil, but the lilac hue speedily passed into a dirty slate colour.

12. Seal oil, with which iodine in the proportion of half a grain to an ounce of oil had been carefully triturated, was exposed to the test. There was but slight action, the iodine appearing to protect the oil from the action of the acid.

13. Seal oil, with which iodide of potassium had been carefully mixed in the proportion of three grains of the salt to an ounce of the oil, submitted to the same test, gave very much the same reaction as unmixed seal oil.

14. Seal oil, mixed with fresh ox-bile, in the proportion of three drops of the latter to an ounce of the oil, was converted into a sort of emulsion, and gave only the reaction peculiar to seal oil. The same result followed when the mixture had been kept a month, and shaken up at frequent intervals.

15. A portion of seal oil, in which iodine and ox-bile had been carefully incorporated in the proportions already named, gave the same result as that mixed with iodine only.

16. Ox-bile, submitted to the sulphuric-acid test, either pure or mixed with two parts of water, was turned to a dirty green colour.

17. Diluted ox-bile, mixed with a little weak syrup, and exposed to

the sulphuric-acid test, gave a lilac reaction, very nearly resembling that afforded by cod-liver oil.

18. Diluted ox-bile, containing a few drops of acetic acid, on the addition of sulphuric acid in excess, yielded a reddish-purple, much less rich and characteristic than that afforded by syrup.

The result of these experiments appear to prove that the pale cod-liver oil is the purest, being that which is least changed in the course of manufacture. This, at least, applies to the oil extracted from the livers of the species of cod met with in British waters and on the banks and shores of Newfoundland. The darker varieties of oil would seem to owe their colour and the greater thickness which they usually possess to the mode of manufacture; probably to the degree of heat employed in their extraction from the liver. It is also probable that the quality and colour of the oil vary according to the length of time the livers are kept previous to the extraction of the oil, for they very soon begin to undergo decomposition.

Sulphuric acid appears to be a perfectly reliable test of the excellency of cod-liver oil, a very little practice enabling a tolerably careful manipulator both to distinguish whether the article is genuine, and likewise whether it has been carefully prepared from perfectly fresh livers and at a moderate temperature. It would appear, as one result of my investigations, that no artificial compound of fish oil with iodine or bile can be readily manufactured so as to simulate the reaction of genuine cod-liver oil with sulphuric acid. The experiment with the liver seems to be decisive as to the lilac hue being the distinctive reaction that pure and well-made cod-liver oil ought to yield with sulphuric acid; and that with the bile seems to prove that this peculiar characteristic is chiefly, if not altogether, due to the biliary elements contained in the oil.

To several pharmaceutical chemists I am indebted for the particulars of the plan upon which the oil is extracted. All agree in the opinion that the freshness of the liver is a most important element towards the manufacture of a pure, sweet, and almost tasteless oil—an opinion fully confirmed by the proof of the rapidity with which it undergoes decomposition afforded by my microscopical investigations. At whatever hour the livers are received from the purveyors, the oil is at once manufactured. The most usual mode of preparation is to put the sliced livers, after being carefully cleansed, into a suitable vessel, to which the heat of hot water or steam is applied. The oil, which is yielded freely, is then strained, and ought to require no kind of purification. In cool weather cod-liver oil deposits its stearine in a flaky form, which, diffused through the oil, gives it a milky appearance. Moderate warmth re-dissolves it, and renders the oil perfectly transparent and as pure-looking as before. During this state, patients find the oil more nauseous and liable to cleave to the palate in an unpleasant manner, and probably on this account the stearine is occasionally extracted. The removal of the stearine does not impair the efficacy

of the remedy, neither is there any reason to believe that it increases it, for I have witnessed equally good results from both kinds. As, however, the stearine contains a large proportion of the ingredients which occasion the lilac reaction with sulphuric acid, I am disposed to believe that its extraction, excepting for the use of very fastidious persons, is, commercially speaking, a mistake, as tending to lessen the quantity of oil procured from a given weight of liver, and thus to increase the cost of an article of so much importance in a remedial point of view, and the price of which is a serious hindrance to its proper administration in the diseases of the poor. It would seem that season exercises a great influence over the quantity of oil yielded by a given weight of livers, the largest produce being obtained in winter. The best time for commencing operations is said to be the end of October, at which time the yield is very moderate. The quantity obtained gradually increases through November, and attains its maximum in December; after January the quantity again declines, and in the spring becomes so small as to render the manufacture unremunerative. During the height of the season, the amount of oil extracted very nearly equals half the weight of the liver employed. One manufacturer informed me, about the middle of December, that from 2301 pounds of liver received during the present season, he had extracted 900 pounds of oil. At that time the yield was just attaining its maximum.—*Lancet, Feb.* 17, 1855, *p.* 182.

33.—*Cod-liver Oil with Quina.*—[Mr. Bastick makes this preparation as follows:] The oleum morrhua cum quinâ is simply a perfect solution of quinine in cod-liver oil. The quantity of quinine may be varied according to the wish of the prescriber, although it is generally employed in the proportion of two grains to each ounce of oil. This preparation is best made in the following manner:—The requisite quantity of disulphate of quinine is dissolved in distilled water, with the aid of a little dilute sulphuric acid. The quinine is precipitated from this solution by means of an alkaline carbonate; the precipitate is treated with boiling alcohol; the resulting alcoholic solution, after being filtered, is evaporated to dryness. The residue, which is pure quinine, is then added to the cod-liver oil, and the mixture is heated in a water-bath until solution is completely effected, which is known by the oil becoming perfectly transparent.—*Lancet, Feb.* 3, 1855, *p.* 126.

DISEASES OF THE ORGANS OF DIGESTION.

34.—ON SARCINÆ VENTRICULI.

By Dr. Arthur Leared, Physician to the Royal Infirmary for Diseases of the Chest, &c.

[In a case of this kind, treated by Dr. Leared, the vomit consisted of a small portion of watery fluid followed by a gush of thick brown

matter, varying from a pint to a quart, which, on standing some hours, indicated active fermentation, the surface being raised and covered with bubbles. The microscope showed numerous sarcinæ.]

Ordered, ℞. Liquoris calcii chloridi ʒj. aquæ ʒvij. Sumat haustum talem ter quotidie.

May 26. Six weeks have elapsed since the above was prescribed; she has not taken it regularly, but is, nevertheless, much better. After the first few days I could discover no sarcinæ in the vomited fluid. The vomiting had been reduced to an average of not more than once daily, and this improvement has continued. Since then she has scarcely had any pain or appearance of the tumour.

She was to-day admitted under my care to the Royal Infirmary for Diseases of the Chest. A daily register of the case was now kept, of which the following is an abstract:—

On the first five days she took the liq. cal. chlor. as before directed; vomiting occurred daily, with one exception, after dinner (at one p.m.), at intervals of from one to four hours—average three hours and a-half. It occurred on two nights also.

On the eleven succeeding days, she took a drachm of hyposulphite of soda in a little barley-water thrice daily, there being few omissions, with these results:—First and third days, no vomiting. On the nine remaining days it occurred from one to five hours after dinner—average, two hours, fifty minutes. On three of the latter days, however, it occurred at four a.m. also; and on two of these at nine p.m. and ten p.m. respectively. The medicine was omitted, as she said it made her weak.

On the three succeeding days she had no medicine. On the first she vomited at four p.m.; the second, at four a.m., and at nine p.m.; the third at four a.m., and again at half-past four p.m., although she had no dinner.

For the next fourteen days she took the following:—

℞. Infus. rhei., infus. gent. aa. ʒss., fiat haust. unam talem ter indies ante cibum sumend.

First day, vomited at half-past four a.m. (medicine not commenced with), again at five p.m. Vomiting after dinner occurred on six days; longest interval six hours and a half; shortest four: average, five hours ten minutes. It also occurred on two other days, times not noted; but on five of the fourteen days there was no vomiting.

The following pills were now substituted:—

℞. Creosoti ℥iss., ext. opii gr. ½, ext. gent. q. s. M. ft. pil sumat. unam talem ter quotidie ante cibum.

These were also taken for fourteen days. They appeared at first more beneficial than the draughts, especially in diminishing the quantity vomited. Vomiting occurred but six times during the above period. The entire cessation of vomiting on the last three

days of it appearing to be caused by her now evidently sinking condition, they were omitted.

From the date (June 12) to her death (June 16), although she continued to take food, there was no vomiting. Death ensued slowly from exhaustion.

[On a post-mortem examination the stomach was found as follows:]

Stomach.—This organ was completely dislocated. It extended perpendicularly from the diaphragm, against which its cardiac extremity pressed to a line that would connect the anterior superior iliac spines. Lying entirely to the left of the spinal column, it occupied the position of the descending colon, which at first sight it closely resembled. It contained about a pint of "coffee-grounds" fluid. Its greatest length, when empty and laid flat, was $11\frac{1}{4}$ inches. Central width $2\frac{1}{4}$ inches. Capacity, measured by distension with water by means of a funnel in the œsophagus, the pylorus being tied, exactly a quart. The coats of the organ, for about three inches from the pylorus, were transformed into a thick unyielding mass. On section this was found to be cancer of the colloid variety, with some fibrous intermixture. It presented under the microscope the usually received evidence of malignancy; and Dr. Bristowe, Pathologist to St. Thomas's, who was kind enough to examine the morbid growth, agreed with me as to its nature. The cancerous mass terminated abruptly in a ring of nodules, some as large as marbles projecting into the stomach; beyond this the mucous membrane appeared quite healthy; but the muscular coat, near the diseased part, was hypertrophied. A test tube 5-12ths inch diameter was with difficulty passed through the tortuous passage representing the pylorus portion of the stomach. The tract was almost completely denuded of mucous membrane.

With regard to remedies, it would appear from the foregoing that chloride of calcium was more efficacious in restraining the vomiting than hyposulphite of soda. Full doses of the latter were given, in order that it should have a fair trial. Its beneficial action was at first more apparent than subsequently; but I observed this to be the case with all the remedies tried. It is to be remarked however, that from the first exhibition of chloride of calcium, the frequency and severity of the vomiting became permanently diminished; and the entire cessation of pain is fairly ascribable to the same agent. I constantly questioned the patient while in hospital in reference to pain; and it is an interesting fact, that, during that period, she suffered none whatever. The relation between the non-appearance of the tumour, cessation of pain, and diminution of vomiting, was, I think, however, clearly established by the position in which the stomach was found. Improvement of these symptoms coincided with diminution in the quantity of the distending gas.

Although, so far as I am aware, chloride of calcium is now, for the

first time, recommended for affections of the stomach, I had previously used it extensively in dispensary practice. In cases of atonic dyspepsia, with acidity and foul eructations, its use is attended with excellent results. Many old sufferers have assured me that no remedy had proved so beneficial to them. I explain this, from its decomposition by many acids likely to be present in the stomach, either abnormally or in excess, their being precipitated thereby, and also to the evolution of free chlorine. To the latter, the destruction of sarcinæ is perhaps, to be attributed. From close microscopic observation during the exhibition of both, I believe this power to be possessed at least as much by chloride of calcium as by sulphite of soda. The point on which I lay stress at present is the permanent improvement of symptoms after liq. cal. chlor. had been a short time taken. Was some catalytic body, distinct from that concerned in the fermentation which subsequently occurred in the stomach, eradicated by this agent?

On omitting the above remedies sarcinæ soon re-appeared, and were invariably afterwards found in the vomited fluid. I also noticed, both associated with sarcinæ, and as constantly when they were not present, a profusion of irregularly-rounded, semi-transparent bodies, of a greenish brown colour, smaller than the sarcinæ, but of variable sizes, more or less aggregated in masses. The constancy, similarity, and general appearance of these bodies made it impossible to confound them with debris of food. I cannot but regard them as embryonic sarcinæ, and that influences destructive to the maturer organisms only arrested their development. This would explain the rapidity with which sarcinæ are sometimes re-produced after apparent total destruction. According to Frerichs, sarcinæ are developed from primitive round cells.

The effect of remedies on the vomiting was marked by its aggravation on their omission. But that sarcinæ were not its essential cause was plain, because the greatest improvement was effected by remedies, with the use of which sarcinæ co-existed.

Different kinds of diet were tried, but with less difference of result than might be expected, and great difficulty was experienced in restraining the craving of the patient for certain articles. Animal food appeared most suitable. Vomiting was always increased by a bulky diet, as bread and broth or gruel. Constipation became at length very unmanageable; castor oil was latterly fancied by the patient as a purgative, but it required the addition of croton oil to insure effect. The suspension of the renal secretion was remarkable. On two occasions the quantity of urine passed in twenty-four hours is noted as only two and a-half ounces; yet the skin was constantly parched. Diuretics were tried, and seemed ineffective. Compared with the thirst and dryness of skin in diabetes, the absence of thirst in this patient was interesting. It points to an intimate relation between perception of thirst and increased renal action. In the

present case, there was simple abstraction of pabulum from the secreting organs. From a comparison of the quantities of urine passed with the quantities vomited in twenty-four hours, on eighteen occasions, accurately noted, I find a very close inverse relation between them. The urine was generally dark coloured, of high specific gravity, and of variable re-action, occasionally depositing whitish lithates. It was often so charged with urea that a very copious crystallization resulted on addition of nitric acid. I have frequently observed this excess of urea in cases of chronic copious vomiting.—*Med. Times and Gazette, Nov.* 18, 1854, p. 517.

35.—*Gallic Acid in Pyrosis.*—Dr. Bayes says that in pyrosis, where this disease is unaccompanied by extensive ulceration, or organic malignant disease of the stomach, or by disease of the liver, the most marked benefit will follow the use of the remedy. Gallic acid, here, not only checks the secretion with a certainty and rapidity he has never seen follow the administration of any other remedy, but it gives general tone to the stomach, increases the appetite, and (what I very little expected when I first used it) in many cases removes constipation. This I can only account for on the supposition that the relaxed atonic state of the stomach which favours pyrosis is continued throughout the alimentary canal, the constipation in these cases arising from want of power in the muscular coats of the intestines to expel the fæces. This want of tonicity is remedied by gallic acid.—*Monthly Journal of Med., April* 1855, *p.* 344.

36.—*Expulsion of the Tænia Solium by the Ætherial Extract of the Male Shield Fern.* By Dr. J. H. Bennett, Edinburgh.—[Professor Bennett has found this to be the best remedy for Tape-worm. The following remarks on a case of this kind illustrate his views:]

She was ordered ℈ij of the ætherial extract of the male shield fern, to be followed in the morning by ℥j of castor oil. This caused the evacuation of seven joints of the worm, each of which were longer than they were broad. Another ℈j dose of the extract was ordered at night, also to be followed by ℥j of castor oil in the morning. Nov. 22. Only three joints of the worm passed. To have this evening ℥ss. of the extract. Nov. 23. This morning, after taking the oz. dose of castor oil, she passed many separate joints, and several long portions of tænia. The whole together, when measured, was calculated to be about fifteen yards long. One portion was evidently formed of the joints of the worm near the head, as they were broader than they were long, and not above the tenth of an inch in breadth. Some joints were square, and others longer than they were broad, measuring from half an inch to three quarters of an inch in length. No head could be discovered, though carefully searched after. She remained in the

house till the 6th of December; but although she took ℥ss. of the extract three times, and one dose of ℈ij, no more joints of the worm came away. This woman has returned to the hospital twice since her dismissal, to say that she is quite well.—*Monthly Journal of Med., Feb.* 1855, *p.* 107.

37.—*On the East and North East Winds, and their Connection with some Derangements of the Gastro-pulmonary Mucous Membrane.*—[Dr. C. B. Garrett, in a work which he has justpublished on this subject, recapitulates his opinions as follows:]

I believe that the east and north-east winds dry up the skin and respiratory mucous membrane; that they arrest the removal of lactic acid by the skin; that the mucous membrane becomes loaded with this acid; that they set up the lactic acid diathesis. That lactic acid is generated to excess in the capillaries; the alkalinity of the blood is overpowered; that sugar and farina undergo acidulous conversion, and that the mucous membranes elaborate lactic acid in excess. That the pneumogastric nerves sympathetically connect the respiratory organs with the stomach, and that other organs also are powerfully affected by the lactic acid diathesis. Under these circumstances, fogs, and an atmosphere loaded with moisture or floating extraneous matters, may also distress the sensitive larynx. The principles of treatment are to restore the healthy action of the skin, to neutralise free acid in the stomach as well as in the blood, and to procure the proper alkaline ascendancy in the circulation. Lastly, to prevent a recurrence, and insure a permanent cure by the *strictest attention* to diet. I may conclude by saying, that if I was forced by any strange coincidence to make my choice in the treatment of diseases (not immediately threatening life) with the use of medicine or the regulation of diet, I would infinitely prefer the means placed at my disposal by the latter, and should rely with the greatest confidence on the powers afforded me by the vast resources of dietation.—*Med. Times and Gazette, Jan.* 20, 1855, *p.* 63.

DISEASES OF THE URINARY ORGANS.

38.—ON DIABETES.

By Dr. Thomas Hodgkin.

[Dr. Hodgkin seems to have paid great attention to cases of this description. What he says is quite true, that in this, as in most other intractable diseases, the patient rarely continues under the same medical man throughout the affection. Hence Dr. Hodgkin has been careful in many cases to watch the disease throughout. He says,]

The exciting causes have appeared to be very various; sometimes the disease seemed traceable to mental fatigue or anxiety; sometimes

to profuse perspiration checked; sometimes to the decline of life; and I have little doubt that in young persons it may have been induced by errors in diet, though instances of this kind are not so easily detected.

With little, if any, exception, I observed the decline and death of diabetic patients to be characterised by what seems to indicate a remarkable loss of tone or vital conservatism of the tissues. They were defective in the power of repairing injury, or gave way to destructive inflammation and sphacelus inducing the death of the patient. It is therefore difficult to say what would be the extreme of the diabetic tabes, if it were not curtailed by the intervention of these local causes.

In illustration of this remark, I may mention, first, the case of sphacelus after venesection, to which I have already alluded, and the fact that the young man whom I have likewise noticed as the subject of another case, was carried off after a very few days of acute pneumonia induced by comparatively slight exposure. On examination of the body, a considerable portion of pulmonary texture surrounding a solitary and solid tubercle was found in a state more resembling gangrenous softening than any of the ordinary stages of pneumonia.

Another gentleman beyond the middle period of life, in whom the disease had probably existed for some years, its progress having been checked by assiduous care, was, by an accidental circumstance, exposed to the cool air of the evening at the close of a warm autumnal day. He was almost immediately seized with pneumonia, which was so very rapidly fatal, that I can scarcely doubt that its characters resembled those of the preceding case, although no examination of the body was made.

On referring to inspections made by other persons, as well as to my own, I find additional examples of similar acute lung disorganisation.

Another gentleman, of similar age, whom I saw occasionally during the course of his malady, but who ceased to be under my care towards its close, was, I was informed, cut off by sores in one or both feet, having a gangrenous character, and proving speedily fatal.

In another case, which I had the opportunity of observing for several years, and which was the most protracted case of well-marked diabetes which has come to my knowledge, the feet were long in a very troublesome state—often threatened with the formation of sores which were only warded off by the most assiduous care, which was paid not only to every indication of commencing lesion in the lower extremities, but also to every symptom in his variable and progressively declining health. At length, symptoms of decided gangrene occurred in one toe, progressively extending to the whole of the anterior half of the foot, and having very much the character of dry senile gangrene. Its progress, as well as the annoyance attending it, appeared to be mainly counteracted by the careful exclusion of the air by means of genuine Barbadoes petroleum. I think it could not have

been less than six or seven months that the patient survived after gangrene had decidedly commenced. Another indication of the impaired conservative vitality of the textures is seen in the tendency to the production of sores on the glans and prepuce from the irritation of the urine.

Reflecting upon the evident tendency of diabetes to impair the vitality of the textures, I could not but attribute it, not to accident, but to an essential connexion between the disease and the function of nutrition; and it occurred to me that there was a great analogy between this state of the system produced by diabetes and that which is met with as an occasional endemic, in some parts of the continent, from the use of damaged rye; both these diseased states being characterised by favouring the production of gangrene. There is, however, this necessary difference in the tractability of the two, that one is produced and maintained by faulty vegetable aliment supplied to the system, and ceases or yields when a wholesome diet is substituted: whereas the other depends on defects of the material of nutrition, produced by imperfection in the process to which materials, good in themselves, are subjected.

Not having had the opportunity of myself observing the symptoms and progress of ergotism, I am unable to say how closely the analogy may be traced; but I should like to commend it to the attention of those who may have the opportunity of making the investigation. It is now eight or nine years since I suggested this view of the subject in my lectures delivered at St. Thomas's Hospital.

Although far from undervaluing the interesting researches of animal chemists for the elucidation of the disease, I was so forcibly struck with the fact that the therapeutical suggestions derived from them, whether in medicine or in hygeia, had signally failed in producing the sired result, that I determined to proceed on a different tack. Instead of restricting patients to animal food, I was content to allow it decidedly to predominate, being at the same time associated for the most part with such vegetable matter as could not yield a large proportion of sugar. The succulent vegetables and well-fermented beers were therefore preferred; though the restriction was by no means strictly enforced, except when an extraordinary increase in the specific gravity of the urine rendered temporary rigid restriction desirable; in these circumstances, it appeared to me to be decidedly useful. In one case in particular, the greatest advantage resulted from a careful member of the family being instructed frequently to take the specific gravity of the urine, and thereby to be able to decide when to indulge and when to restrain. The stomach was therefore not needlessly offended; and the preservation of health was promoted by a somewhat extended range of diet.

The example of the Edinburgh patient, so remarkably benefited by returning to bodily labour, induced me to insist strongly upon the use of daily exercise. But, in conjunction with the promotion of perspir-

ation so effected, I judged it necessary to guard against its suppression by vicissitudes of temperature, by means of flannel clothing, and to maintain the healthy condition of the skin by sponging and friction. I was particularly impressed with the desirableness of this part of the treatment, by the notorious fact that the kidneys and skin are to a considerable degree vicarious to each other, and also by the observation that diabetes appeared in some cases to be traceable to the suppression of a very active cutaneous perspiration. Moreover, in diabetic patients not only is there a sweet odour of the breath and urine, but the perspiration also partakes of the same character. A diabetic patient has himself been struck with the return of the normal odour of this secretion amongst his symptoms of improvement. Another point to which I thought it necessary to pay strict attention was the character of the alvine secretions, which, as I have already observed, are apt to be not less perverted from their normal character than the urine. A greatly perverted and offensive odour, and a more or less complete absence of bile, were the most evident morbid characters. Whilst they appeared to me strongly to forbid the employment of opium (much as this drug has been recommended), they pointed to the use of occasional alterative doses of mild mercurials, and induced me to give ox-bile in such a manner that it might be blended with the food on leaving or when it had just left the stomach. The benefit derived from this last suggestion was so marked, in a very protracted case which came partially under my care, that the medical man who was in close attendance constantly had recourse to it to relieve the *malaise* which was experienced whenever the absence of bile became most complete.

The medical agent which I have been accustomed to regard on the whole as the most important, has been creasote, given in very small doses (half a minim) at the time of taking food. It appeared to me that the greatest error in the process of assimilation is in that part which should take place at its commencement, viz.:—in the stomach and duodenum; and it is in stimulating these that creasote in such doses seems mainly to act. In large doses it would probably be too powerful, and would disturb rather than assist; whereas, in very small quantities, it probably produces that diffused slight excitation which, as Billard has shown, is characteristic of the digesting state of the healthy stomach.

In conjunction with these means, I have thought it desirable to use such other vegetable or mineral tonics as seemed best suited to the state of the individual patient. Strychnine, quinine, and cascarilla, are those which I have preferred. About eight years ago, I attended a patient in the north of England, who presented the symptoms of well-marked and far advanced diabetes. He was emaciated, but his loss of power was still more remarkable. I laid down for him a course of management as to medicine, diet, and exercise, in accordance with the principles which I have just explained. I never saw him

afterwards; but I had the satisfaction of learning that his improvement was great and unequivocal. He recovered the power of walking several miles, but I had no opportunity of learning whether there was any restoration of the sexual power, which he had entirely lost. After the lapse of a considerable time, the patient's health again declined. He came to town to consult me, when I was unfortunately absent; and I was not informed of his state and symptoms. He necessarily came under other hands, and ultimately sank; but I heard no particulars regarding his death.

I had the opportunity of watching for many years a much valued friend and patient, who was long the subject of this malady. It had been recognised and treated for a considerable time before the case came under my observation; it fact, it might be said to have then arrived at a fully confirmed stage. The mode of treatment which I have described was carried out in every particular. The urine was frequently and carefully examined; and, though it fluctuated in its specific gravity, it for the most part but little exceeded the lightest diabetic point. The urine was also frequently submitted to careful chemical analysis by an experienced manipulator, who was purposely kept in ignorance of every particular respecting the patient, except that he was diabetic; and I regard it as no small confirmation of the beneficial influence of the means employed, that he volunteered the the opinion that the specimen of urine was voided by a young person in whom the disease was slight and incipient, and that he also suggested that the diet employed was too exclusively animal, although, as I have before observed, it had by no means been my practice to subject diabetic patients to severe restriction in this respect; and the gentleman in question who was advanced beyond the middle of life, had formed no exception to this rule. I may further add, that this patient continued to be corpulent, and, for a considerable time, varied but little in weight. Even towards the close of life, the diabetic symptoms did not increase, unless some tendency to tenderness and soreness in the lower extremities might be regarded as such. Great hepatic derangement and gastric symptoms apparently connected with the gouty diathesis of this patient, may be said to have nearly or altogether superseded the original malady.

In another case, occurring in a person connected with one of the government offices, diabetes was fully established. The urine was copious, saccharine, and heavy; and thirst was proportionately great. Under treatment of the character which I have already described, all the symptoms yielded, and the patient improved in health, strength, and appearance; and, as he assured me, he still retained his virile power. Notwithstanding this encouraging change, the patient died suddenly, and, if I am not mistaken, in a fit; but, as I neither attended him myself, nor received any information from the medical man who did so, I am unable to give any particulars of this event, except that it was quite unexpected.

I might mention some other cases which have come under my care; but they offered little if anything to add to the points which I have already noticed. I have not sought for diabetic patients, and have never taken up the subject as a speciality. Had I done so, I should have been prepared to present a very different communication from that which I am now making. Contemplating the disease with interest, not merely in consequence of the remarkable character of the symptoms which it presents, but also in relation to other diseases, it suggested considerations which, in my opinion, possess interest and importance in relation to pathology and therapeutics in general. Although I have not the vanity to suppose that I have done much towards the cure of a disease generally regarded as incurable, I may at least hope that I have taken a short step in a right direction towards the legitimate object, viz., that of mitigating its symptoms and deferring its fatal result. It is also some satisfaction to believe that this may best be done by discarding some hitherto applauded means which are deleterious or distressing.

In the pathology of the disease, I believe that the first perceptible indication of derangement of the healthy process occurs in the stomach. I have long entertained this belief; and, on turning to the authorities who have made a study of the subject, I find that I am borne out by M. Bouchardat, and also by Dr. West, an American physician. But are we hence to suppose that there is any organic derangement in the stomach itself? The morbid anatomy of the stomach has thrown no light on the subject. Daily experience teaches us how much the functions of the stomach depend on conditions not primarily connected with the organ itself. Mental influence can instantly destroy the best appetite; a blow on the head may give rise to nausea and sickness; and loaded intestines may produce intense acidity. As we may palliate and mask this acidity by an alkali, so we may temporarily suppress or modify the condition of the stomach as it exists in the diabetic at the commencement of their digestion. The effect, however, is only palliative, not curative; and to make any progress towards obtaining this result, we must employ all those means which indirectly yet effectually improve the stomach of the dyspeptic, viz., relief from care, cheerful but not engrossing mental occupation, bodily exercise, fresh air, attention to the skin, and a due amount of rest. A trainer would perhaps be the best hygeist for a diabetic patient.

The curious experiment of Bernard, who has shown the powerful influence of the irritation of a particular part of the brain in the production of sugar in the liver, seems to have a bearing on this subject, and to favour the view which I have taken; since it can hardly be doubted that the influence on the liver in the one case, and on the stomach in the other, was exerted through the *par vagum*. It is not improbable that, even in Bernard's experiments, the sugar was really produced in the stomach, and thence conveyed into the circulation.

The presence of sugar, in some form or other, in the animal system, is not inconsistent with a healthy or physiological state. It is perhaps always more or less present in the liver, especially in that of the dog; hence probably picromel in the bile. It is always present in milk; and all the farinaceous articles of diet appear to be partially dulcified in the process of digestion. It should not be so much our object wholly to banish sugar, and those vegetable principles which are convertible into it, as to maintain the function by which its production and conversion are regulated. It is probably from an imperfect view of the whole subject that we may be led by accurate chemical observation to an erroneous chemical theory in treatment; hence, inconsistent as it may appear, I should have no hesitation, under certain circumstances, to employ acids in the treatment of a dyspeptic patient, though troubled with various symptoms of acidity; or to use castor oil, or cod-liver oil, or oily emulsions, or to allow a moderate quantity of some fatty aliments where they might seem to be called for, although I might have reason to suspect fatty degeneration in some particular organs.

Although in diabetes I attach the primary importance to the digestive organs, and more especially to the stomach, liver, and pancreas, and to some undetermined condition of the nervous system by which these organs are influenced, I am very far from limiting the interest to these organs. The kidneys also claim attention; and great importance is doubtless to be attached to the peculiar cachectic condition superinduced, very probably through the kidneys, by the disease.

I am induced to differ from Dr. West, and others who may regard the kidneys as only passively concerned in the disease, by the following reasons:—In three or four instances I have met with diabetic patients, in which the disease occurred in families in which Dr. Bright's disease of the kidneys also occurred; and in two of these examples, the family predisposition to this latter affection of the kidneys was very strongly marked. Again, the disturbance of the function of the kidneys does not seem to be limited to the mere passing off of the sugar brought to the kidneys by the blood; since, though urea is found in diabetic urine, it appears to be far below the healthy proportion. There is another symptom which, though not immediately connected with the kidneys, will strengthen the presumption that they are materially concerned. The great diminution or total abolition of the sexual power, so intimately associated with this disease and so characteristic of it that a patient has told me that it was the first symptom which called his attention to this malady, cannot be ascribed to mere general debility; seeing that the symptom occurs when the patient, as respects the general powers of the system, is very much stronger than many advanced phthisical patients, and other extremely debilitated subjects, whose virility remains perfect. I conceive, therefore, that there must be some sort of reflex action upon

the organs of generation, of which the starting point is in the kidneys. The idea does not appear so be an overstrained one, seeing that the renal and spermatic plexuses are so intimately connected with the same portion of the spinal cord.

Again, it seems scarcely possible that the kidneys could be subjected with perfect impunity to the constant passage of their secretion, inordinate in its quantity, and vitiated and morbid in its properties. Finally, the resulting cachexia is precisely such as is to be expected from the imperfect performance of the depuration of the blood by the kidneys, and should direct our attention to them, just as a livid countenance prompts us to look for derangement in the organs of respiration.

I would in the next place invite attention to the state of the tissues of the body in general, which constitutes the cachectic condition to which I have alluded. It seems to result from the combined action of the diffusion of the sugared blood over the system, and the imperfect elimination of those excrementitious matters which the kidneys are destined to throw off. I have already compared it to the effect produced by the employment of diseased rye. It ought also to be compared and contrasted with the cachexia resulting from Bright's disease of the kidneys, to which it must be considered as allied; and, to do the subject ample justice, it should be considered in conjunction with some other forms of cachexia which merit careful attention, and which require to be studied as the complement to the morbid anatomy of all the organs considered individually. To attempt to do this, even in the most cursory manner, would carry me beyond the limits which can be allowed to this paper.—*Assoc. Med. Journal, Oct.* 13, 1854, *p.* 916.

39.—*On the Curative Treatment of Chronic Morbus Brightii.*—[In a paper read before the Harveian Society on this subject, by Dr. Handfield Jones,]

Four cases were related, in which patients, who suffered under dropsy, with more or less of general cachexia, and an albuminous condition of the urine, which contained casts, &c., were restored to greatly improved health by treatment steadily continued. At the same time the quality of the urine improved in a corresponding measure, and this very gradually. The treatment consisted in the administration of iron, chiefly the muriated tincture, either alone or combined with the bichloride of mercury. Cod-liver oil was given in one case, and quinine and the sulphate of iron in another. The position taken was, that Bright's disease was essentially a degeneration, widely remote from inflammation, and that its cure was to be attempted by the employment of all means likely to raise and maintain the general power. In a fifth case, after the removal of considerable ascites and anasarca, at first by blue pill, squill, and digitalis, and afterwards by the tonic treatment above described, the latter was con-

tinued with great improvement to the general health, but the urine still contained albumen. As, however, there was found no deficiency in the amount of urea or uric acid in the secretion, according to a rough estimate, the opinion was expressed that the draining off of albumen probably depended on an altered state of the Malpighian capillaries, similar to that which exists in cases of (so-called) chylous urine, the renal functions still performing their functions fairly well. In conclusion, it was strongly urged that practitioners should always carefully look out for indications of commencing renal degeneration, and strive rigorously and perseveringly to arrest the morbid process by such measures as in the cases above detailed had proved successful. The disease was compared to pulmonary phthisis, and the hope expressed that, as medicine was more adequate in many instances to the arrest of the one, so it might become also to that of the others.—*Lancet, March* 31, 1855, *p.* 343.

40.—ON DIABETES.

By Dr. R. H. Goolden.

[An anonymous writer in the 'Med. Times and Gazette' recommends that in this disease we should give the salts containing oxygen in feeble combination, such as the chlorate of potassa, or, still better, the permanganate of potassa. Dr. Goolden has, however, failed in seeing any advantage from these medicines. In his interesting paper read before the Harveian Society, he gives a series of cases, in which cerebral disease and injury were the direct causes of diabetes, and with the relief to the cerebral mischief by treatment directed to the brain alone the diabetes vanished.]

The first case, which suggested all my subsequent experiments and observations, was that of a railway guard, stunned by a blow from the handle of a break, when revolving with force. Concussion and diabetes ensued. Since then, I have been able to trace the commencement of the disease to injuries. One patient had fallen down stairs, struck the back of his head, and was stunned, nine months before admission; the same date he gave as the commencement of his diuresis. Another had been stunned by the fall of a piece of timber from Victoria Tower, which injured his arm, and he had been diabetic ever since. This man was improving, but left the hospital, because he would not have a seton; but is, I understand, now in St. Bartholomew's, with the seton. Another man, now in King's Ward, traced his disease to a kick on the head from a horse, from which he suffered severely at the time. I might relate several others, were it not that it would occupy too much of your valuable space.

Another group of cures has presented itself, whose history does not record any accident, but which have been cured by treatment directed

to the brain. I have only three of these, a small proportion to the whole, but invaluable as to their importance for induction.

Another important group consists of cases where the cure has been spontaneous or accidental. I have seen diabetes in its most aggravated form subside within a few hours; but pulmonary deposit, ending in a vomica, supervened.

Mr. Clapton, our very able resident assistant apothecary, who has watched these cases with great care and interest, informs me of a case of a medical man in the country, who was obliged to part with a large practice on account of being the victim of diabetes, and unable, from his consequent debility, to pursue his profession. After suffering great emaciation and debility, a large anthrax on the nape of his neck occurred, which extended towards the occiput, and down the back, and was nearly fatal to him; but, thanks to port-wine, brandy, and quinine, he recovered not only from the carbuncle, but also from the diabetes, and is now quite well.

Mr. Amphlett, of Birmingham, informed me of a case of diabetes in the person of a wealthy manufacturer, whom he treated upon the old plan of animal diet for some months, without much benefit, when he was suddenly seized with an attack of apoplexy; for this he was purged and cupped, after which there was no more diabetes. He subsequently died, as I learn, of a second attack of apoplexy.

In contrast with these cases, I have met with many that have not been cured by anything I could do for them; but, in the larger number of such cases, the urine has diminished at least one-half in quantity, and the specific gravity sunk eight or ten 1000ths, notwithstanding the patients have been supplied with abundant farinaceous food, cane-sugar, and as much fluid as they liked to drink.

I must also mention, that pulmonary disease, or congestion, is not an essential part of diabetes, though it does frequently coexist. In two aggravated cases now in King's Ward, the lungs are quite clear. Dr. Bristowe and myself took some pains in examining them, to ascertain the fact, in consequence of your correspondent's assertion, which, if universally true, would be an important point.

With regard to remedies, our attempt at specific treatment must prove a failure. The supply of oxygen to the food in the stomach can and does do no good. The stomach cannot be made to perform the function of the lung; but, whatever the suspended function may be, I have found it partially restored by the application of the continuous galvanic current from the nape of the neck to the epigastrium. In one chronic case, which I had not been able to relieve to any satisfactory amount, I found the quantity diminish one-half after the battery had been applied for half an hour for three days; and I am now about to try what may be effected by the use of Strongfellow's or Pulvermacher's portable electro-voltaic batteries.—*Med. Times and Gazette, Dec.* 9, 1854, *p.* 598.

41.—ON DIGITALINE.

By Dr. R. Christison, Professor of Materia Medica in the University of Edinburgh.

[No observations seem to have been made in English works on digitaline, the active principle of digitalis, although this is one of the best diuretics which we possess, its greatest fault however being its uncertainty. On the action of digitaline Dr. Christison says :]

My experience of its effects has been highly satisfactory. I have used it expressly as a diuretic only ; but its sedative virtues have also come incidentally under observation ; and it is quite evident that digitaline is a most energetic remedy in both ways.

In the first two trials made with it, which were both of them in cases of extensive, protracted, obstinate œdema in connection with disease of the kidneys, diuresis commenced, in one towards the close of the second day, and in the other a day later ; in both the flow of urine was profuse ; and in both the œdema entirely disappeared, but with the slowness not uncommonly observed in this form of dropsy.

Here I cannot help observing in passing, that a striking illustration was presented, on both these occasions, of the unsoundness of the objection which has been brought by many practitioners, since it was first propounded by Dr. Osborne, against the use of diuretics in the renal forms of dropsy. I have taken every opportunity, in my clinical lectures, and in occasional publications, to protest equally against the unsoundness of the theory and the looseness of the facts, which have led to the prohibition of diuretics in renal dropsy, and which have deprived many persons of the most immediate and most efficacious means of relief from the principal secondary affection occurring in Bright's disease of the kidneys. And I may here repeat, in the strongest terms, that I have never, except in a single instance, seen the albuminosity of the urine, or any other indications,—which either the other conditions of the urine or any local symptoms can furnish,—increased under the employment of such diuretics as I have given in this disease. These are digitalis, squill, and bitartrate of potash, sometimes singly, more commonly combined. What may be the case with other diuretics, I shall not pretend to decide. I strongly suspect that they are all in the same category, and that they have been shunned on grounds purely theoretical and baseless. But at any rate I can answer for the theory not holding in the case of the three diuretics I have mentioned. So, too, in the case of digitaline. It is not a little remarkable that in the first two patients to whom I administered it, the albumen, instead of being increased, was quickly and greatly diminished. In one it disappeared entirely in a few days, and did not return so long as the patient remained under observation in convalescence. In the other it also disappeared ; but after some days the albumen reappeared, though in a greatly diminished proportion. Digitaline, therefore, while it stimulates the kidneys to increased

secretion, has not, in so doing, any effect, as some have thought of digitalis and diuretics at large, in increasing the peculiar renal irritation which constitutes or occasions Bright's disease.

Digitaline has proved equally, or even more serviceable in dropsy connected with disease of the heart. It has, in my hands, accomplished complete discharge of the dropsical effusions, and thus effected such relief as to enable the patient to return to his occupations, though given in circumstances apparently very desperate. In this respect digitaline has done no more than is often accomplished by foxglove itself. But it has appeared to me to act with more speed, and with greater force after the action did begin. In one instance great depression of the heart's action was brought on instead of a flow of urine.

I have also given it in some local dropsies, more especially ascites, either simple or combined with anasarca of the lower part of the body only. It has not proved more useful, however, as a diuretic in these cases than digitalis and other diuretics ; which in general fail to influence the urine or remove the dropsical effusion, when given internally. I have not yet tried it externally, according to the method recommended by me with infusion of foxglove.

There can be no doubt that digitaline possesses the action of foxglove itself upon the heart and circulation. I have not made express trial of this action. But while using it to excite diuresis in a case of dropsy with diseased heart, the pulse began to flag, soon fell to 44, and became very soft and somewhat irregular. At the same time languor, nausea in a slight degree, and faintness ensued. All these symptoms disappeared in a few days after the remedy was discontinued. On this occasion I observed that the two actions of digitaline, its diuretic and sedative actions, do not concur. It is very likely that they are even incompatible. This I have formerly stated to be a probable fact in regard to the actions of foxglove. The disregard of it is, I apprehend, the reason why some underrate the efficacy of foxglove as a diuretic. Attention should therefore be paid to the fact in using digitaline.

The dose to be administered is stated by MM. Homolle and Quevenne at a seventy-fifth part of a grain of digitaline, three times a day. I have never given any other dose. It is a very small one certainly. But a tenth of a grain will kill a little dog ; so that the dose is only in proportion to its exceeding energy. The form I have used is that of a pill about a grain in weight. It is to be given with the conditions and precautions observed in using foxglove.—*Monthly Journal of Medicine, Jan.* 1855, *p.* 2.

42.—*Treatment of Enuresis.*—As it is often desirable to have at hand various formulæ for the treatment of this troublesome affection in children, we may mention that Dr. Blaschko, of Freyenwalde, re-

commends a mixture of equal parts of *tinct. nucis vomicæ* and *tinct. ferri acet.*, of which from 10 to 15 drops are given twice during each evening. Dr. Hüber, of Zurich, recommends *ext. nucis vom.*, 1 p., *oxyd. ferri nigr.* 48 p., giving 2 grains night and morning. Nägele gives 1 grain of *tannin*, night and morning.—*Gaz. des. Hôp.* 1855, No. 22.—*Med. Times and Gazette, March* 24, 1855, *p.* 289.

43.—*Use of Cubebs in Infantile Enuresis.* By Dr. Deiters.—This author has found cubebs more effectual than any other remedy in curing the incontinence of urine so common among children. This complaint may depend on atony of the bladder, or on the presence of intestinal worms. In the former case, the cubebs acts as a tonic, in the latter as a valuable anthelmintic. The medicine requires to be given in considerable doses; two pinches (*i. e.* a few grains or *Zwei Messerspitzvoll*) for infants, and half a teaspoonful twice or thrice daily for children of a somewhat more advanced age. Its effect is speedy and permanent; and although occasionally it happens that during its administration the incontinence returns at periodical or irregular intervals, these recurrences gradually become less frequent, and eventually disappear altogether. To effect a radical cure, the author has often found it necessary to continue its use for a period of from three to eight weeks, and he has never observed any injurious effects from its administration.

Deiters observes that he has found the same remedy most efficacious in checking nocturnal emissions in cases of spermatorrhœa.—*Preus. Varein Zeitoch.—Dub. Hosp. Gazette, Feb.* 15, 1855, *p.* 32.

SURGERY.

AFFECTIONS OF BONES, JOINTS, &c.

44.—ON CONSERVATIVE SURGERY.

By Henry Smith, Esq.

[Mr. Henry Smith, in a journey partly for health and recreation, visited the Jersey Hospital, the scene of Mr. Jones' labours. He gives us some interesting remarks on what he saw.]

Both in the Jersey Hospital, and in various parts of the Island, are many living instances of the remarkable success which has attended Mr. Jones in his Conservative efforts.

Of the cases of excision of the knee-joint I had an opportunity of seeing three, one of which only is under treatment. The patient being a girl about 15, who had suffered for several years with incurable disease of the joint, which had made serious inroads on her constitution. The operation was performed about three months since, and although recovery had been much retarded by a large abscess which had formed on the back, the case is doing admirably; the line of incision had healed; the patient can move her limb, which was in capital position; and she was anxious to get up, as she thought she would be able to move about. This is the only operation of the kind since my visit last year.

The other two cases are those where operations were done about two years ago. The one is that of the little boy who was brought over to London by Mr. Jones last winter, and whose case excited much interest, in consequence of some morbid condition existing in the hip on the other side. Much difference of opinion was expressed by various eminent surgeons, as to the precise state of that joint—some saying that the head of the thigh-bone was dislocated, others that it was not. Now, however, there is no doubt, for the disease is in an advanced stage; and with all the signs of dislocation, the head of the thigh bone can be distinctly felt to be thrown upwards and forwards; the parts about the knee, on the other side, are perfectly healthy, there being a very firm anchylosis, and the limb itself has increased considerably in length, and is perfectly straight.

The third case is that of a boy also, aged about 15, whom I saw last year, then perfectly well. He is now a fine active youth, and has a firmly anchylosed knee; by means of an apparatus which Mr. Jones has contrived for him, he can move about with the greatest agility

without any stick. In fact, his activity is so great that he is obliged to be restrained—as illustrations of which I may mention, that on one occasion the young fellow was caught in the topmast of a vessel in the harbour, and another time he was espied in the branches of a mulberry tree in the garden of the hospital. In Park's celebrated case of excision of the knee-joint, the patient could run up aloft, he being a sailor. Mr. Jones's little patient has, it will be seen, accomplished the same feat with his stiff knee.

I had not time to see the other three cases in which Mr. Jones has successfully excised the knee-joint, but I believe they are all well.

More remarkable and perhaps more strikingly illustrative of the benefits of conservative surgery than any other case, is one in which excision of the entire ankle-joint, together with about three inches of the fibula, had been performed by Mr. Jones. It is impossible to describe accurately the extraordinary result which has taken place here. The patient was an adult, who had some six months since received a compound Pott's fracture. Attempts were made to save the entire limb, but so much mischief was produced, both locally and generally, that Mr. Jones determined, against the opinion of all, to excise the ankle-joint. This operation was done, and the man made an excellent recovery.

On looking at the limb, if it were not for the cicatrix across the front of the joint marking the line of incision, it would be not easy to know that any such operation had been done, for the parts look almost natural, and there is a movable joint, and so little shortening that the patient can walk about, with or without his boot, briskly. The wound is perfectly healed; and what is extraordinary is, that in so short a time the portion of the fibula which was removed has become regenerated. The operator is very proud of this case, and well may he be. I have possibly had the opportunity of seeing and being engaged in as many operations upon joints as any other young surgeon, but I must candidly confess that this ankle-joint case is beyond anything I have ever seen in that way.

From the ancle we went to the *wrist;* and here Mr. Jones has had splendid success in two cases, where he performed excision of the entire joint. In the one, the patient is a healthy-looking young woman, who had suffered long from strumous disease of the carpus. Excision was performed some twelve months since, and the consequence has been a perfect cure. There is a completely stiff and well-looking joint; the girl has very fair use of the fingers, and she exhibited it to me with a great air of pride and satisfaction, the more especially perhaps, as she recognised in me the same individual who, twelve months ago, saw her completely disabled, she at that time having just undergone the operation.

In the second case, the patient is a woman, about 40. The operation has been done for disease about six months; a complete recovery

has taken place, and there is a stiff wrist, but, as yet, there is very little ability to move the fingers; for this, more time is required.

Of the Elbow.—Mr. Jones showed me one case where he performed re-section with the most satisfactory results, the patient being a woman, about 50 years of age, and the operation having been executed about two years since for disease. Notwithstanding the patient's age, a capital result has followed, for she has a good movable joint, and can exercise with facility the not very easy duties of cook at a large house. I may here mention, that it is somewhat rare to see a movable joint after excision of the elbow; and, moreover, that the operation in question does not generally turn out very well in persons of advancing or advanced life. Therefore, I look upon this case as the more interesting and remarkable.

Among other cases in the Hospital, I saw still under treatment a patient from whose foot Mr. Jones had removed a greater portion of the os calcis and astragalus for disease of the tarsus. This case is bidding fair to do well.

A case was shown to me presenting an admirable illustration of the result of amputation at the knee-joint, the patient being a little girl, who, some months since, had received a terrible injury of the leg, extending to the knee, and wounding the popliteal artery. Amputation was done through the knee. The result has been a perfect recovery, and the little girl has as good and as long a stump as can be seen in any Hospital.

Among other instances I saw in this little Hospital, was one which may be truly ranked with the best efforts of conservative surgery. It was that of a poor little boy, about 8 years of age, whose left arm, from the shoulder as far as the lowest point of the elbow, had been completely bound down to the side of the chest by firm adhesions, the the result of a burn. By a series of surgical manœuvres, the arm has been completely liberated, and the boy in my presence clasped both hands over his head. The wounds made by the surgeon are perfectly cicatrised.

I saw also an illustration of the result of resection of the hip-joint; but it was in the form of a pathological preparation in the little Museum attached to the Hospital. It consisted of the parts about the hip, removed after death from a female adult, who had died from disease of some internal organ a year or two after excision of the head of the femur had been effected by Mr. Jones.

This preparation shows the upper end of the thigh-bone resting against the ilium, just above the site of the acetabulum, which has become partially obliterated, and bound down to it by a dense and tough fibrous tissue, forming almost a complete capsular ligament. The patient could, after the operation, make use of this new joint with considerable facility.

The cases above alluded to indicate pretty tolerably the advance which Mr. Jones has made in conservative surgery during the last few

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or three years. The success which attends his operations is remarkable. There has been, I believe, only one fatal result after these operations, the case being one where he performed excision of the knee-joint; and here death was due to a violent dysentery, with which the patient was attacked during an epidemic raging in the island. It is evident that the very great success met with here is in a measure due to the salubrious air of the place.

It will be seen, by consulting the records of surgery during the last one or two years, that the opposition to excision of the knee-joint is not maintained with that violence which formerly held; for the operation in question has been executed by several London and provincial surgeons, and with such an amount of success, that the mortality arising from it has not been so great as that attending amputations of the thigh; and there are doubtless many who will not allow their minds to be prejudiced by incautious critics against this proceeding, but will test this operation in well selected cases of disease of the joint, when such opportunities occur.

It is not, however, to be expected, that in London such success should be met with as has attended Mr. Jones in Jersey, where everything so favourable to the recovery of patients obtains—cheap and wholesome diet, and, above all, pure air; nevertheless, the very excellent results which have followed these efforts in conservative surgery in the hitherto (professionally speaking) obscure island of Jersey, have doubtless induced many to cultivate that branch of their art with more assiduity; and possibly this brief sketch of what I have lately witnessed there, may stimulate others to inquire into fairly, and investigate in all its aspects, a subject which is worthy of the best attention of every surgeon.—*Med. Times and Gazette, Nov.* 4, 1854, *p.* 475.

45.—*Condition of a Joint after Resection.*—Mr. Henry Smith exhibited to the Medical Society the bones which had been removed from the lower extremity of a boy who had two years previously undergone excision of the knee-joint by Mr. Jones, of Jersey, had entirely recovered from the operation, and had died from disease of the liver a few weeks ago. A firm, bony anchylosis had taken place at the site of operation, between the extremity of the femur and the tibia, in nearly a straight position. The patella also, which had not been removed in the operation, was united to the anterior part of the femur, and its ligament was still attached to it, and to the tibia, so that altogether a very firm, compact mass of bony tissue occupied the situation of the knee-joint. This preparation demonstrated that bony union does take place between the ends of the bones after resection of the knee, and that a most useful limb may be saved. In the instance of this boy, as in that of others he had seen and heard of, there was great facility of walking. The patient from whom this most instructive specimen was removed was the younger of the two

boys who had a year ago been exhibited to the Medical Society, and the fellows had seen for themselves how well he was enabled to use the limb. There was at that time some disease of the hip on the other side, and, since his death, it was discovered that dislocation upwards had occurred. This case was a sufficient answer to the objection that had been raised, of making the limb, by the operation, "like a flail."

Mr. JONES said he had no remarks to make, except to strongly urge upon surgeons, in cases of operations for diseases of the knee-joint, to save the patella, and not divide the ligament. When these parts were saved, there was less difficulty in walking than when they were removed.

In answer to a question, he said the boy from whom the preparation had been taken had not suffered from any tubercular disease, but had died from extensive disease of the liver, spleen, and kidneys. Two months before, he was quite well, and walked about with ease.—*Lancet, Dec.* 9, 1854, *p.* 484.

46.—ON EXCISION OF THE ELBOW-JOINT, &c.

By JAMES SYME, ESQ., F.R.S.E.

[Mr. Syme says, that twenty-five years ago, in all cases of incurable disease of the elbow, the established practice was amputation of the arm. This might have been so in Edinburgh, and even in London, and most other places, but from the testimony of Mr. Smith, senior surgeon of the Leeds Infirmary, we believe that excision of the elbow-joint was an established practice in that hospital for many years before that period, although the practice was not published in any book or journal. Mr. Syme, however, certainly has the credit of popularizing this important operation, and could not have known what was going on in Leeds when he himself perhaps was only a student. Mr. Syme attributes the ill success which attended the early operations of this kind, by Sir Philip Crampton and others, to an error in regard to the extent of bone requiring removal. He says,]

In caries, little more than the articular surface is engaged in the disease, and the part thus affected is of very limited thickness; but beyond this the bone is roughened, being covered by an effusion of osseous substance, which is not morbid any more than that thrown out to unite a fracture, or supply the loss of substance in necrosis. Moreau, father and son, were the surgeons who had performed the operation in France. Moreau says, in speaking of one of his operations, that, finding the bone roughened, he cut off another inch of the shaft. Sir P. Crampton, for a similar reason, removed the bone "three inches above the tuberosity of the inner condyle." The effect of such proceedings is, to produce so great relaxation of the parts, as to allow the muscles little command over the limb, while at the same time the

recovery is much protracted. It occurred to me that the results might be better, if the part really diseased were alone removed. In 1827, I had under my care a young gentleman of tender years, with disease of the elbow-joint, which had resisted all treatment. I wished to excise the articulation, but not finding any one in Edinburgh to sanction so unprecedented a proceeding, I reluctantly performed amputation. I had not seen him again till last year, when a tall, fine-looking man entered my room, and introduced himself as the subject of this case. I confess, I felt some remorse of conscience on looking at his empty sleeve; but I had the satisfaction of reflecting that this was the last case in which I was guilty of any such unnecessary mutilation. In 1828, I performed the operation on my own responsibility, and in 1831 published a treatise on the subject, having in the interval operated on fourteen cases. This mass of evidence attracted attention, and notwithstanding very violent opposition, the operation has since become more and more generally adopted, and at last even in London, which held out longest, it has during the last few years been occasionally practised. But while thus adopted, it has shared the same fate as another operation to which I lately alluded, the principles of its performance having been overlooked. There is no operation in which the mode of making the incision is of more consequence. Unless they be sufficiently free, the portions of bone cannot be removed without risk of injury to the remaining parts, and unless they be properly directed, the resulting cicatrices will interfere with the free motions of the limb; and we are not satisfied here with the result unless there be perfect flexion, extension, and rotation, with the full natural amount of strength. I believe the only incisions by which this perfect result can be obtained are those originally practised by Moreau, which are, a transverse one, carried across the back of the articulation immediately above the olecranon, from the ulnar nerve to the external condyle, and two longitudinal through the extremities of the transverse, so that the incision altogether has the form of the letter H. When the flaps have been raised, free access is gained to the articulation, and when the operation is completed, the edges of the transverse incision, if brought accurately together, generally adhere by first intention, instead of healing by granulation and adhering to the bone, so as to prevent free motion. Having dissected up to the flaps, do not attempt to remove all the articulation at once, as otherwise there will be a risk of cutting the ulnar nerve, with consequent deficiency of sensation and atrophy of the limb; but having exposed the olecranon, cut it off in the first instance, to as to get free access to the joint, then divide the external lateral ligament, and having pushed the ulnar nerve over the inner condyle, free the end of the humerus and saw it off on a line with the tuberosities; and, lastly, remove in succession the ends of the radius and ulna on a line with the base of the coronoid process. More than the extent thus defined would be unnecessary and injurious, while less would hardly remove

the disease, and, even if it did, would incur the risk of anchylosis. In the cases you have happened to see there has been anchylosis, resulting in one from disease, in the other from injury, and therefore there was some deviation from the ordinary plan of proceeding, but its general principles were kept in view.

The operation of excision of the elbow was not employed for anchylosis till long after it had been adopted for disease, since I had operated more than a hundred times for the latter before I did so for the former. Though often requested to excise the joint under these circumstances, I had not been prepared to interfere with the healthy integuments merely for the convenience of the patient. But at last a young man, from the Highlands of Perthshire, came to consult me with both arms permanently extended, one immovably so, and the other admitting of but a very slight degree of motion at the elbow; resulting, in the one case, from fracture with dislocation, in the other from dislocation only. It is impossible to imagine a greater degree of helplessness than this young man's, who was obliged to seek assistance in the performance of the most ordinary offices. The case called loudly for interference, and I performed excision of one elbow. The result was an arm so perfect that it could not be distinguished from a sound one till his sleeve was removed, exciting some surprise at the Medico-Chirurgical Society of this city, where I exhibited him several years ago.—*Lancet, May* 3, 1855, *p.* 231.

[Mr. Smith, senior surgeon to the Leeds Infirmary, denies that Mr. Syme was the first to introduce this operation into this country. The following extract from our 10th Retrospect, and the additional note from Mr. Smith to the Editor of the 'Lancet,' will explain this matter.]

"In '*Retrospect*,' vol. x., p. 134, Mr. Syme says—'Previous to 1829, the operation of cutting out the elbow-joint had not been performed in Great Britain.' Since this paper was published, we have received a note from Mr. Smith, senior surgeon of the Leeds General Infirmary, from which we take the following extract, to show that the operation was performed in Leeds, by Mr. Stansfield, as early as 1817 or 1818. Mr. Smith says—

"'In 1818, I was town surgeon here, and had charge of the workhouse. During that time, a man about twenty-four years of age, named Jeremiah Butterworth, a chimney-sweep, died under my care, in the workhouse, of consumption. Knowing that excision of the elbow-joint had been performed with success upon him at the infirmary a year or two before, by Mr. Josiah Stansfield, then junior surgeon of the infirmary, I was anxious not only to examine the joint, but to preserve it as a pathological specimen. I asked the Board, and (in consequence of my explanation of the importance of the case) obtained its permission to remove the limb, which I took to my own house. After satisfying my own curiosity by dissection of

the limb, I presented the bones of the new joint as a trophy to Mr. Stansfield. This gentleman died in the following January. After his death, Mr. J. P. Garlick had possession of them. I succeeded Mr. Stansfield at the infirmary in February, 1819. At that time excision of the elbow-joint was a recognised operation there, and had been performed by both my seniors,—Mr. Chorley and Mr. Hey. For a time it fell into disuse; but has been performed by both my present colleagues, Mr. Hey and Mr. Teale, as well as myself. The last operation I performed, the case was so unfavourable, that I was recommended, when the operation was half done, to finish by amputation; but I determined to risk the case, and had the satisfaction of succeeding, the cure being complete, with a very good power of flexion, extension, pronation, and supination.'"

I have this week called on Mr. J. P. Garlick, who was house-surgeon to the infirmary, in 1818; and he empowers me to say he has a perfect recollection of the case of Jeremiah Butterworth, and remembers its success.

There is another operation which Mr. Syme claims, and which goes by his name—the Division of Stricture by Incision in the Perinæum for the cure of Stricture and Fistula in Perineo. That operation also has been performed in all proper cases by the surgeons of the Leeds General Infirmary for forty years past. I have, however, never seen it performed in cases where a grooved staff could be previously passed into the bladder, as we have generally been able to cure such cases by milder measures, without the necessity of so severe an operation.—*Lancet, March* 17, 1855, p. 300.

[Mr. D. R. Rankin says that Mr. Syme's historical account of excision of the elbow-joint is imperfect. He states as follows:—]

Previous to Mr. Park's cases of excision of the knee-joint, and his suggestions as to excision at the elbow-joint, (1782, translated and published in France, 1784,) the latter operation (excision of the lower end of the humerus) had been performed by Mr. Wainman, of Shripton, 1760; and again, twenty years subsequently, Mr. Justamond, of London, removed the head of the ulna. Moreau, came on the stage in 1786, and here Mr. Syme commences his history.—*Lancet, March* 31, 1855, *p.* 349.

47.—*Excision of the Elbow-joint.* By W. Fergusson, Esq., F.R.S. —[Mr. Fergusson operated more than two years ago on the patient whose case is here commented upon. The man now enjoys a fair amount of motion in the elbow-joint, although the elbow had been previously anchylosed in a very awkward position, and was useless.]

In that state the patient had applied to various hospitals, as also to a bone-setter, (who twice disturbed the anchylosed surfaces.) When

the man came before Mr. Fergusson, the latter thought the only way of dealing with this stiff joint was to follow the method of Mr. Barton, of America, who had cut out a wedge-shaped piece from the front of the tibia, and created, in some degree, a new joint below the anchylosed knee. Mr. Mackenzie had applied the same method to the elbow, and he (Mr. Fergusson) had thought that this operation was the surest means of giving some mobility to the stiff elbow-joint of this patient.

He accordingly had cut out a large portion of the end of the ulna, radius, and humerus, and the amount of bone taken away was rather considerable, as there had been much hypertrophy of bone, or rather formation of new osseous tissue. The operation was, in fact, a resection of an anchylosed joint. The subsequent treatment was simple, and the patient had left the hospital with the wounds cicatrized, and some motion in the joint. Now, two years after the operation, the mobility of the arm was considerably increased, and it was satisfactory to have a living proof of the great amount of benefit which may be obtained by the adoption of decisive measures in cases of this kind. The patient had been reported to be dead, but he stood here to prove the contrary.—*Lancet, Nov.* 25, 1854, *p.* 436.

48.—EXCISION OF THE KNEE-JOINT.

By Dr. Wm. Keith, Senior Surgeon to the Aberdeen Royal Infirmary, &c.

[This patient was aged 14, and of a scrofulous constitution.]

Physical Signs.—The right extremity is wasted, the leg drawn up and fixedly bent at a right angle to the thigh. The knee is greatly enlarged, tender to the touch, and incapable of being moved in any degree, the attempt causing much suffering; the skin over the whole limb, leg, knee, and thigh, is so dry and scurfy, as to present very much the appearance of icthyosis; there are two openings in the ham, one on each side of the popliteal space, from which a sero-purulent fluid is constantly discharged, and both openings communicate with the knee-joint.

History.—He states that seven years ago, when jumping from a dyke, he sprained his right ankle-joint; this was followed by a succession of abscesses, the last of which only healed up at the end of three years of constant suffering. Continuing well and at school for two years and a-half, he met with an accident eighteen months ago, which, bruising the knee, was followed by much pain and swelling of the joint. With little done deserving the name of treatment, six months of confinement to bed elapsed, when abscess from within the joint found exit by one of the openings in the ham, after which he was so much relieved as to be able to go out on crutches, his knee

bent up as it is at present. The opening being very imperfect, a fresh collection of matter formed soon after, and burst at the opposite side of the ham, and these for twelve months have continued to discharge daily, the joint all the time much enlarged, tender to the touch, and more or less painful on movement up to the present time. Pulse never under 120.

On consultation it was agreed to excise the joint.

Wednesday, 17th May, 10.15 a.m.—Under chloroform; the joint was opened by the semilunar incision of the late lamented Dr. Richard Mackenzie. In lifting up the flap so formed, osseous union, partly accomplished between the femur and patella, had to be broken up. 1 inch and $\frac{3}{8}$ths were sawn off the femur, and $\frac{5}{8}$ths of an inch from the tibia, making in whole 2 inches of bone removed. The surface of the patella was pared and smoothed, and the gelatinous, degenerated, synovial membrane peeled and dissected from the face of the flap. Three small vessels were tied, the limb straightened, and the wound closed by six stitches, was dressed with ointment, compresses, and a many-tailed bandage, and laid into a well-fitting Macintyre's metal fracture-frame. After being put to bed, 20 drops of laudanum were administered. To have arrowroot and brandy for dinner. 9 p.m. —Stomach irritable, vomited the arrowroot, retains port wine and water. Limb lies comfortably, feels the frame press hard on the outside of the knee, which was at once remedied by a soft pad of lint. It added also to his comfort to have the heel set entirely free from pressure, accomplished by passing a long thickish pad under the tendo-Achilles. To repeat the anodyne at bedtime.

Oct. 30th, 166th day.—He walked to the hospital, a distance of nearly a mile. The limb is all but straight, the joint stiffly anchylosed, he lifts the limb about with freedom, and that without the slightest uneasiness. While walking he rests on the toes with confidence. The heel wants $2\frac{1}{4}$ inches to touch the ground when he stands erect with the pelvis squared, showing the height of heel which his boot will require to enable him to walk comfortably. The whole limb is plump, and he himself florid and healthy to look at. He has grown sensibly in height and stoutness since the operation. and he congratulates himself much on having saved his limb, whether it keep pace with his growth or not—a point that can be settled satisfactorily only after some years' observation.

Remarks.—No worse subject than the above John Reith could well have been selected for any major operation. A constitution saturated with struma, weakened by long suffering, and his body wasted by copious purulent discharges. The knee-joint disorganised, and the soft parts in the ham permeated in various directions by sinuses constantly distilling pus, yet on the complete removal of the one great source of irritation, viz., the diseased knee-joint. the constitution was enabled, by the aid of a generous diet, to rally, to re-establish healthy action, and repair every breach.

To the remarks appended to my former case, published in the 'Monthly Journal of Medical Science' for April 1854, p. 329, I would only add, that while performing the operation, it were well to clear away from the face of the flap the whole of the synovial membrane; healthy it cannot be, but whether healthy or not, bony union is desiderated, and any secreting surface of a serous character will retard, and cannot facilitate either adhesive union of soft parts, or *ossification of bones.*

Secondly. Much attention should be given to the direction of the saw when cutting through the femur and tibia, that these bones be cut on planes parallel to each other, else they cannot meet face to face when the limb is fairly extended. All important as it is for early union that cohesion should commence from the hour they come into contact, the line of section in the femur should be close to the edge of the articulating cartilage of the inner condyle, which will remove all the articular surface likely to be implicated in disease, and still leave a breadth of bony surface about equal to the cut surface of the tibia. The more nearly of one and equal extent the better for present union and for after use. I must again repeat, the patella is indispensable, if after strength or comfort is desired, and that no portion of flap should ever be pared off or lost in any way. Skin is always useful, and when even superfluous, is wonderfully retractile and accommodating.

In conclusion, it may be safely affirmed that the nature of John Reith's constitution, and the state of the soft parts around the knee, were alike unpromising prior to the operation; yet on the seat of disease and the source of constitutional irritation being removed, nature's powers were speedily called forth, and have accomplished healthy and solid union of bone to bone, so that the patient could walk with little assistance on the 99th day after excision of the joint. The present opportunity may be taken to state, that John Hay, the subject of the former operation, is at this date skin-whole and sound, walking nimbly on a stiff straight limb, miles in a day, without the aid of staff or cane.—*Monthly Journal of Medicine, Dec.* 1854, p. 524.

49.—ON CHRONIC INFLAMMATION OF THE HIP-JOINT.

By Wm. Coulson, Esq., Surgeon to St. Mary's Hospital.

[Hip-joint disease, of a chronic nature, is, perhaps, the most important of all articular affections. Mr. Coulson prefers the term chronic inflammation of the hip-joint, which may commence in and for some time be confined to any of the tissues of the joint. The early symptoms may easily be mistaken at first.]

At this stage, then, the only symptoms essentially connected with the disease are, pain and impeded motion, and these become modified

or of greater intensity as the complaint progresses towards the second stage. The general health is frequently unaffected, though this, of course, will depend upon the state of the patient at the moment of attack, while the presence or absence of constitutional disturbance will mainly depend on the low or active nature of the inflammation at the onset. At this period we do not discover any deviation from the natural condition of the affected limb; it is of the same length as the other; the nates are not altered in form, except under circumstances which I shall presently notice, and the spine is perfectly straight. Still, after some time, it will be observed that the patient, while walking, has a tendency to carry his limb straight, as if there were no joint in the knee, and to incline the foot either inwards or outwards. These are slight symptoms, but they should always awaken attention, and if a careful examination of the joint be now made, we shall rarely fail in discovering that it is the seat of disease.

I have already explained to you the best mode of conducting your examination in cases of suspected hip disease. Abduction of the limb generally excites more or less pain in the affected joint; but to avoid all source of error you should examine the patient in an horizontal position, on a flat, hard surface, as a table, &c. Grasping the foot of the affected limb in one hand, and placing the other hand on the knee, you rotate the head of the femur against the acetabulum, and the increased pain which the patient experiences will apprize you of the mischief going on within the joint. Pressure should always be made with the hand on and behind the trochanter major, and in front, where the psoas and iliacus muscles pass over the articulation.

From the structure of the hip-joint, and the difficulty of avoiding undue pressure between the articular surfaces of the bones, you can readily understand how, as the disease progresses, the patient employs every effort to diminish the pain and distress arising from the necessity of throwing the weight of his body on an inflamed and, perhaps, ulcerated surface. The greater part of the symptoms attending the second stage of hip disease are connected with this constant effort to keep the limb as free from motion as possible, and various physical signs are now added to the functional derangements just described. The whole limb becomes flabby and diminished in size, from the constrained state of repose of its muscles; but the diminution is best seen in the glutæi muscles, which are so intimately connected with the joint. On looking at the patient from behind, you will at once be struck by the appearance of the nates. The affected buttock always becomes flatter and more wasted than the one on the sound side; the marked division between the buttock and thigh is lost; the buttock on the affected side is lower by an inch or more than is natural, and the whole limb appears to be longer than the one which is free from disease. The wasting and altered form of the nates are easily explained by the forced inactivity of the glutæi muscles; but the real or apparent lengthening of the limb does not admit of such ready

explanation, and has given rise to considerable discussion; some surgeons contend that the limb is actually longer than the other, while some affirm that the lengthening is merely apparent. The truth appears to be, that, in the greater number of cases, the affected limb presents an appearance of elongation; that in a few cases it is really elongated to a slight degree; and, finally, that the second stage may pass over without any change whatever taking place, either in the real or apparent length of the limb.

An actual lengthening of the limb may be produced by effusion of fluid into the cavity of the joint. I have no doubt in my own mind of this sometimes taking place, but it is not of frequent occurrence, and the elongation thus produced seldom exceeds one-third of an inch; the head of the bone cannot be depressed more than an inch without destruction of the round ligament. To understand the nature and causes of the apparent elongation of the limb, which so often characterizes the second stage of hip disease, we must examine the patient in the erect and recumbent posture, and take into account the influence exercised on the limb by the positions which he voluntarily assumes. I have already discussed the different points of this interesting question, but a brief notice of them here may be useful. If we examine the patient in the erect posture, we shall perceive at once that he supports the whole weight of the body on the sound limb; the thigh is flexed on the pelvis; the leg on the thigh; the limb is commonly rotated outwards; the pelvis is inclined, that is to say, depressed, towards the affected side, and at the same time carried forwards, the anterior spinous process being more or less in advance of that on the opposite side. Now, all these circumstances tend to give an appearance of elongation to the limb, which can be produced at will in the healthy body, and which, during hip disease, often makes the affected limb appear from three to three inches and a half longer than the sound one. Indeed, in some cases of disease of the knee-joint, where the hip is unaffected, but where the patient assumes the position just described, the limb has been found apparently elongated to the extent of two inches—a fact which clearly proves, that apparent lengthening may take place independently of any change in the articulation of the hip. The lateral inclination of the pelvis brings with it a corresponding depression of the fold of the buttock; and if you are curious enough to examine female patients more closely than is the custom, you will discover another effect of the same cause. One of the labia—that of course corresponding to the affected side—is much lower than the other. This is very remarkable in young subjects from the close juxtaposition of the parts. The other symptoms which accompany this second stage of hip disease are little more than aggravated states of the pain and impaired functions already noticed.

In the third stage the pain gradually increases in intensity, becomes very severe, and impedes every motion of the limb, particularly that of extension. The effect of a constrained and uneasy position may

probably contribute to increase the original pain in the knee-joint, and from some recent dissections it would appear that the articulation itself is more frequently involved in the disease than we had formerly been led to think. The pain in the hip is often violent during the night, preventing rest; the slightest movement of the joint causes agony, and the patient is either confined to bed or hobbles along with difficulty upon crutches. In many cases, however, even at this advanced period, the pain in the hip is often masked until the precursory symptoms of suppuration make their appearance. Constitutional disturbance is now observed, perhaps for the first time, and ushers in this stage of the disease. The pain in the joint becomes aggravated, and the febrile symptoms continued; there are frequent shiverings, with painful startings of the limb; the rest is greatly disturbed, and the patient experiences great distress from starting and catching during sleep. Suppuration now takes place, sometimes in the cellular tissue external to the joint, sometimes within the joint itself, in which latter case the capsular ligament soon gives way, and the effused matter gradually works its way to the surface. It is not always easy to distinguish whether the suppuration has commenced in the cellular tissue or in the joint; but, however this may be, it sooner or later comes to the surface, and is either discharged by a natural opening or with the aid of art. In many cases inflammation attacks the cellular tissue interposed between the glutæi muscles; and the nates, which were formerly wasted, become tumefied and painful; at a particular point the skin now inflames, fluctuation can be distinguished, and if an opening be not made, the abscess discharges itself by ulceration. In other cases it descends along the outer and back part of the thigh, or an intra-articular abscess, having perforated the capsule of the joint, may take one of the various directions which I have pointed out in a former lecture. From the abundance of cellular tissue in the neighbourhood of the hip-joint, the quantity of matter discharged is always considerable at first, and it may soon give rise to hectic symptoms; but it is astonishing to what an extent patients are sometimes reduced, and yet recover. Arrived at this stage, the disease may terminate in one of two ways: the symptoms may gradually decline; the discharge of matter becomes less and less, until it ceases altogether, the fistulæ heal up, the patient's health is slowly restored, and a cure is at length effected by anchylosis of the diseased joint. Or the symptoms may become aggravated, and signs of destruction of the joint then set in. During this period the various lesions which I have described in a former lecture succeed each other with greater or less rapidity, and by reflecting on their nature and extent you can understand without any difficulty many of the symptoms by which they are accompanied. In order to avoid repetition, I may briefly state the different changes of structure now alluded to. The ligamentum teres is ulcerated and gradually destroyed by absorption. The capsular ligament also is thickened or softened, relaxed in some places, per-

forated in others, and at an advanced stage of the disease often almost entirely destroyed. Ulceration of the cartilages sometimes precedes effusion of pus into the joint, sometimes appears to be the effect of purulent inflammation of the other tissues. However this may be, the articular surfaces of the acetabulum and head of the femur are in all cases more or less damaged by ulcerative absorption, the extent of the destruction varying in nearly every case. Thus only small portions of the cartilaginous lining of the joint may have been removed, or the acetabulum is rendered deeper and wider by ulceration, while the transverse and cotyloid ligaments have likewise given way. In the head and neck of the femur every degree of destruction may be observed. The cartilage, for example, is either partially or wholly absorbed, loose portions of cartilage may be found in the cavity of the joint; or the head, and even the neck, of the femur may be more or less destroyed by ulceration and caries, with or without scrofulous deposit in the cancellous tissue of the bone.

Chronic inflammation of so large and important a joint as the hip is always a serious disease, and it must be admitted that, in the majority of cases the prognosis is unfavourable. Still, many circumstances will modify this opinion. Thus the disease is much more manageable when it attacks young children and persons below fifteen or twenty years of age, than when it occurs in adults. This may depend on the circumstance that the disease in children is usually of a scrofulous nature, for scrofulous inflammation of the joints in young subjects, even when accompanied by abscess and extensive destruction of the joint, has a much greater tendency to heal than when the disease occurs at a later period of life. I have seen many children recover who laboured under unequivocal symptoms of hip disease in its various stages; and the experience of the surgeons attached to the Children's Hospital in Paris confirms this opinion of its curability, even under the most discouraging circumstances. That form of the disease which commences in the cancellous structure of the bone has, I believe, a greater tendency to terminate in anchylosis than the other two forms. The prognosis I should consider peculiarly unfavourable when the disease is attended at an early stage with extensive gelatinous degeneration of the synovial membrane. Any serious constitutional disease co-existing with the local malady must affect the prognosis, and if the patient exhibit symptoms of pulmonary tubercle, the case may be considered as altogether hopeless. The existence of numerous abscesses about the joint, particularly if they communicate with its cavity, is likewise an unfavourable circumstance, for here we have not the resource of amputation, as when other joints are affected. In the latter stages of the disease a favourable termination may be expected if the abscesses begin to heal and signs of commencing anchylosis present themselves.

The diagnosis of hip disease in its various stages is surrounded by many difficulties, for surgeons engaged in extensive practice are fre-

quently called upon to examine patients who have been treated for hip disease, when the results of the cases prove that no such disease ever existed. I have seen many examples of this error myself, and you will find various others recorded in works on diseases of the joints; I shall therefore endeavour to explain to you at some length the complaints with which chronic disease of the hip may be confounded, and the characteristic signs by which it may be distinguished from them. The affections which bear more or less resemblance to hip disease are, psoas abscess, caries of the pelvis, inflammation of the sacro-iliac articulations, chronic rheumatism of the hip-joint, sciatica, and hysterical affections of the joint.

You are already aware how, in the early stage, hip disease seldom presents any other symptoms than pain and some slight impediment to motion, and hence you can see at once that nervous disorders are the most likely to simulate this first stage of the disease, because their characteristic sign is pain, and the pain, if severe, entails more or less difficulty of motion. What, then, are these painful nervous disorders? They are sciatica and hysterical affections of the joint. Sciatica is chiefly characterized by pain, so is hip disease at the commencement, but there are several circumstances by which the two complaints can be distinguished without much difficulty. The pain in sciatica becomes very severe soon after the onset of the disease, and though it may be first felt about the hip it soon extends down the thigh to the leg, following the course of the sciatic nerve and its branches. In hip disease, as I have told you, the pain as often commences in the knee as in the hip, perhaps oftener; it is seldom very acute at first, and artificial motion during this early stage never produces the violent aggravation of pain which is excited by moving the limb during a severe attack of sciatica. Again, patients affected with hip disease can go about tolerably well during the first stage, showing slight lameness only, whereas a person attacked by sciatica is soon laid up and quite unable to place the foot on the ground, much less bear the weight of the body. As the diseases progress their distinguishing symptoms become much more clear and decided. After the first smart attack, sciatica has a tendency to diminish, even though no treatment be employed; mere rest alleviates the pain, and in a week or two a manifest remission takes place, whereas the progress of hip disease is steady and onwards; and during the second or third stages no error of diagnosis can be committed.

Hysterical affections of the joint simulate incipient hip disease much more closely. Like sciatica, these affections are characterized by intense pain in the neighbourhood of the joint, together with a real or imaginary inability to move the affected limb; but on carefully examining all the circumstances of the case, you will seldom experience any difficulty in arriving at an accurate diagnosis. In the first place, the patients are almost invariably females; and whenever one of the fair sex complains of intolerable pain in a joint, with little appearance of organic lesions beyond a mere puffiness of the integuments, you may

be pretty certain that her complaint is hysterical, for it has been calculated that at least four-fifths of the females amongst the higher classes of society, supposed to be suffering from diseases of the joints, are, in reality, affected with hysteria, and nothing else. You will not, however, rest content with this general inference, but direct your attention to the concomitant signs of hysteria, and compare any existing symptoms with those that are known to attend true hip disease at a corresponding stage. It is an established fact, that hysterical females are more subject than any other individuals to neuralgia, and it seems to me highly probable that the complaint now under consideration is hysterical neuralgia of the joint. Many of the characters of the pain lead to this conclusion. It is excessively violent from the commencement, and in this respect differs from the pain of true morbus coxarius. The patient also refers it to the hip-joint, and not to the knee, as is so often the case when the disease depends on incipient organic changes. The pain, again, is not limited to the affected articulation, but extends to the buttock and down the thigh, a circumstance in which it resembles sciatica. This last affection, indeed, is often nothing but rheumatic neuralgia. This general diffusion of the pain constitutes an useful distinction between the hysterical complaint and true disease of the joint. The irregularity of the pain, and the peculiar modifications of sensibility which accompany it, are highly characteristic, and at once point to an hysterical origin. The pain, though apparently intense, is not constant; it often suddenly disappears for a time, without any evident cause, particularly if the patient's mind be diverted from her condition; and in like manner, the more the sufferer's attention is directed to the part, the more she complains and appears to suffer. Besides this, the pain is absent during sleep, whereas the rest is always more or less disturbed by the pain of organic disease. The sensitiveness of the skin is truly hysterical; the patient shrinks from the approach of the finger, yet sometimes will allow you to press the joint in all directions, without complaining of much pain. The nates and the limb in general are not wasted, and there is no elongation of the limb. Sometimes, however, from the spasmodic muscular contractions to which these hysterical patients are very liable, the limb is drawn up and inverted, the trochanter major is prominent, the knee bent, and the thigh rests on the sound one. Here the case might, by a superficial observer, be easily mistaken for dislocation; but its history, and the possibility of bringing the affected limb into a natural position, will distinguish the case from one of true displacement of the bone.

The foregoing are examples in which functional disorders simulate organic disease in its early stages; in other cases, the difficulty of diagnosis may depend on the existence of some organic lesion in or near the joint, yet differing in its nature from the changes which constitute true hip disease.

It is evident that the more nearly these lesions resemble in their

nature those of morbus coxarius, and the closer they are to the joint, the more numerous will be the points of resemblance between the two affections. Experience proves this to be the case, and shows that caries of the pelvis, inflammation of the sacro-iliac symphysis, and psoas abscess, are the affections most likely to be confounded with chronic hip disease. When caries affects any portion of the pelvis close to the hip joint, the functions of the articulation are impeded; pain is felt in the region of the joint, and abscesses form in its neighhood; but the general symptoms are always less severe than when the articular cavity is the seat of the disease; the pain, also, is less severe, and not so evidently located in the joint, the movements of which are not very materially impeded. In hip disease, the immobility of the limb, after a certain period, is a constant and characteristic symptom; whereas here, although the hip-joint may be stiff, and a disinclination to movement exist, certain motions can be effected, without inducing any great pain. The suppuration from caries of the pelvis is less abundant in most cases than the discharge from hip disease, and after some time the probe will detect the existence of caries in the bone, when all doubt as to the nature of the disease will be removed.

Chronic inflammation of the sacro-iliac symphysis often bears a close resemblance to morbus coxarius. In children, a true joint, furnished with its synovial membrane, exists at the union between the sacral and iliac bones, and the inflammation is of the same scrofulous character as that which attacks other bones at an early period of life. The symptoms follow the same slow and progressive course as in hip disease. At first the patient complains of pain in the lumbar region; in some cases the pain is seated quite close to the hip, and is referred to that joint; or it may show itself from the commencement in the knee—a circumstance very likely to deceive the practitioner, and induce him to refer the symptom to disease of the hip-joint. Lameness soon follows, and the symptoms, which, as you perceive, are the same as those observed at the commencement of hip disease, gradually increase in intensity. But this is not all; they soon give rise to other symptoms also, analogous to the effects produced by disease of the hip. Thus it will be observed that the patient avoids lying on the affected side: the nates are flattened and altered in shape; the muscles of the limb become wasted; the patient throws his weight on the sound limb; the affected one is apparently elongated, from depression of the pelvis on the affected side; the knee is slightly flexed, and the point of the foot directed either forwards or slightly outwards. As the disease progresses, the symptoms increase, and the elongation of the limb is followed by shortening, which adds another remarkable point of resemblance between this disease and true coxalgia. If not arrested at this stage, abscesses form in the neighbourhood of the posterior spinous process of the ilium, and open either on the back of the thigh or in the groin; the suppu-

ration becomes abundant, and the patient is worn out with hectic. This unfavourable termination is, however, more rare than in hip disease, for the progress of the malady is arrested, or imperfect anchylosis takes place between the opposite surfaces of the iliac and sacral bones.

From this account, you will perceive many points of resemblance between the two affections: we have in both pain and lameness at the commencement; we have the same, or nearly the same, appearances of the nates and limb, the same position, the same lengthening followed by shortening, and the same formation of fistulous abscesses about the upper part of the thigh. Great attention will therefore be required to distinguish inflammation of the sacro-iliac symphysis in a young subject from hip disease, not only at its commencement, but in subsequent stages. The first point to which attention should be directed is the precise seat of the pain. In hip disease, the pain of the joint, when it exists, is most severe in the groin, or just behind the trochanter major; in the other affection, the pain is chiefly developed by making pressure transversely near the posterior superior spinous process of the ilium. When tumefaction exists, its seat is also different. In hip disease, the fulness is at first perceptible either in front of the joint or immediately behind the trochanter major; in chronic inflammation of the sacro-iliac symphysis, we never discover any tumefaction in the groin, but the puffiness or swelling is always seated over the posterior spine of the ilium. In hip disease, the joint soon becomes fixed, and any attempted motion excites intense pain, whereas the thigh can be freely moved while the patient is lying down, in cases of sacro-iliac affections. The limb, in cases of this latter disease, an be extended without much difficulty; and although the patient may walk very lame, he puts the whole of the foot on the ground; while, in hip disease, the toes only touch the ground, and the heel is elevated. Finally, the shortening of the limb in sacro-iliac affections is always apparent, not real; but in hip disease, the shortening depends on destruction of the acetabulum and head of the femur, or dislocation on the dorsum ilii; it is real, instead of being apparent.

Formation of matter about the region of the groin or hip, accompanied by pain and difficulty of motion, might lead to the inference of disease of the hip-joint in its third stage, and cases are recorded were psoas abscess has been mistaken for it; but attention to the following points will always enable you to distinguish the one complaint from the other. In psoas abscess the pain commences in the lumbar region, and is not felt on the groin until the disease has made considerable progess. Rotation outwards of the limb greatly increases the pain, whereas the reverse is noticed in cases of diseased hip-joint; the various positions of the limb, its elongation, shortening, &c., which characterise morbus coxarius, are observed in psoas abscess, and the peculiar effect of position, of coughing, &c., in increasing the swelling

or discharge of matter depending on psoas abscess, is altogether absent in cases of hip disease.

There are still to be noticed two series of cases, one of which may simulate generally disease of the hip, while the other only resembles the third stage of that affection. The complaints to which I now allude are a peculiar shortening of the limb, produced by injury to the hip-joint, and chronic rheumatism, or, as it has been otherwise called, morbus coxæ senilis. It was formerly supposed that all cases of real shortening of the limb depended either on the hip disease which I have thus described, on fracture within the capsule, or on chronic rheumatism. But Mr. Gulliver has described, in the 'Edinburgh Medical and Surgical Journal,' (No. 128, vol. xlvi. p. 97,) some cases of shortening of the neck of the thigh-bone, depending upon other causes, and which are particularly worthy of attention. This peculiar lesion has been hitherto attributed either to fracture within the joint, or to the natural effects of old age; but Mr. Gulliver has clearly shown that it may occur in young subjects, as the result of injury to the joint, and under circumstances which give considerable resemblance to the later stages of hip disease. The history of these cases, as related by Mr. Gulliver, may be comprised in a few words. A young man receives an injury to the hip, from a fall probably, and immediately afterwards complains of pain in the joint. The pain is not very severe, but it continues, and lameness supervenes; these symptoms may go on for many months, the thigh wasting, and the limb slowly shortening, until it becomes perhaps two or three inches shorter than the sound limb.

Cases of the kind described by Mr. Gulliver bear a close resemblance to hip disease in its earlier stage, and at the onset cannot well be distinguished from it; but as the malady progresses the differential signs soon become evident, and a little attention will enable us to discover them. Although the limb is a couple of inches shorter than the sound one, and although the shortening be found to depend on organic lesion, the motions of the joints are perfectly free. Now this could never occur in the ordinary hip disease, and the reason why it it happens in the cases now alluded to is that the synovial membrane and cartilages are quite free from disease. For the same reason also there is little or no pain produced when the opposite surfaces of the joint are brought into contact by pressing the head of the femur againt the acetabulum; there is no tumefaction about the joint, abscesses never form, and the patient remains free from constitutional disturbance of any kind, although the limb is continually getting shorter and the lameness increasing until it becomes incurable. All these circumstances are incompatible with the notion of chronic hip disease, such as I have described it to you; and even at the earliest stage, while the pain and lameness are considerable, we cannot detect on examination any signs of inflammation going on within the joint. I should also mention that the patient, though he limps, put the foot

flat on the ground—another point of distinction between this disease and morbus coxarius.

A short account of the pathology of these interesting cases will enable you to understand better the symptoms by which they are accompanied. The synovial capsule of the joint, and the articular cartilages, are quite healthy; but the neck of the thigh-bone is much shorter than is natural, and forms nearly a right angle with the shaft; or the neck may be removed altogether by absorption, while the head is flattened and expanded. On dividing the parts, the shell of the neck, if any remains, is found condensed, and, in one case, the cancelli contained caseous matter. Sometimes the acetabulum is widened and remarkably shallow, corresponding to the alteration of shape in the head of the bone. These cases have been assimilated to the examples of interstitial absorption of the neck of the femur which takes place as a natural consequence of old age; but, in my opinion, they have a different origin, and should be referred to absorption—the result of osteitis, without any tendency to suppuration.

Analogous, in outward appearance at least, to the foregoing affection, is the chronic rheumatism of the hip-joint, which has been well described by Mr. Robert Smith and Mr. Adams of Dublin. This disease generally affects patients above fifty years of age, though it may occur at an earlier period of life. Its pathological characters are peculiar. The cartilage of the head of the femur is more or less destroyed, not by ulceration, but by a process which seems analogous to interstitial absorption; it looks, in fact, as if it had been worn away by the effects of long continued friction; and the points at which the cartilage is most extensively removed correspond to this notion of wearing down or *usure*, as the French surgeons term it. The neck of the femur is also shortened to a greater or lesser degree, and sometimes so much so that the change produced bears a strong resemblance to fracture of the neck of the bone within the capsule. The os femoris is expanded and flattened, while the acetabulum, which is likewise deprived of its cartilage, is rendered wider and deeper than natural. When the disease has continued for any length of time, the round ligament disappears, and the osseous surface, from which the cartilages have been removed, are covered by the porcelain-like incrustation, which I have described in a former lecture; the synovial membrane is not diseased, nor has suppuration ever existed, I believe, within or outside the joint, but the capsular ligament is generally thickened. Many of the symptoms of this disease are common to it, and to morbus coxarius, and hence the necessity of discriminating between them.

The patient first complains of pain about the joint, increased whenever he throws his weight on the affected limb; the limb itself is apparently shortened to the extent of two or three inches; and this, of course, produces considerable lameness, the limb and foot being rotated outwards. The trochanter major is more prominent than in the natural state, the muscles of the thigh are wasted, but not so those of the

calf of the leg, according to Mr. Adams at least. The nates of the affected side are quite flat, and no trace exists of the lower fold of the glutæus. Here we have several of the symptoms which attend morbus coxarius; but the points of difference are sufficiently numerous to render our diagnosis easy. In the first place, if you make accurate inquiries touching the history of the case, you will generally discover that the patient experiences some rheumatic pains in other parts of the body, or has previously laboured under an attack of rheumatism—in a word, that he is of the rheumatic diathesis. In the next place you will learn, that the pain and stiffness of the joint are relieved by gentle exercise; when the patient has walked about for a short time, the movements of the joint become somewhat more free—a circumstance quite the reverse of what takes place in hip disease, accompanied by shortening, when indeed the patient is totally incapable of walking at all. The pain is not increased by pressing the head of the bone against the acetabulum, and this is a distinctive sign of much value. Finally, on careful examination, you will find that a great deal of the shortening of the limb is apparent, not real; the affected limb may appear to be three inches shorter than the sound one, yet on making accurate measurements of the limb in the manner I have already described, you will discover that two-thirds of the shortening, at least, depend on altered positions of the limb and pelvis. In addition to all this, the constitutional disturbance of true hip disease is never present, nor has the rheumatic affection any tendency to excite the suppurative inflammation which so frequently accompanies the later stages of morbus coxarius. I must, however, observe, that the shortening of the limb in rheumatic cases, which I have just described to you, does not always depend on shortening of the neck of the thigh bone, and deepening of the acetabulum; it may be produced by actual dislocation on the dorsum ilii, and thus is added another remarkable point of resemblance between rheumatic affections of the joint and the ordinary disease.

I have still to consider the treatment of chronic hip disease, but a great deal appertaining to this part of the subject has been already noticed; I shall therefore conclude with a few additional remarks on certain points of treatment which were not sufficiently developed in my preceding lectures. In them I confined my account of treatment to scrofulous affections, what I shall now add is intended to apply to cases of hip disease occurring in persons who are not scrofulous. When speaking of scrofulous affections of joints, I expressed an opinion unfavourable to the use of active antiphlogistic measures in such cases, but this does not apply to hip disease in strong subjects, who present no signs of a scrofulous habit. Here, bloodletting must be employed, or at least blood must be taken by leeches or cupping-glasses from the neighbourhood of the joint. For the quantity of blood to be extracted, no fixed rule can be given; you must have regard to the strength and constitutional powers of the patient, his age, the amount of pain, &c.,

and you must regulate your conduct accordingly. At an early stage of the disease, and in the cases now spoken of, the best general treatment consists in administering calomel, combined with opium, until the mouth is affected. The disease is often arrested as soon as the peculiar effects of mercury manifest themselves; but some patients, as is well known, are unable to bear this powerful remedy, which irritates their bowels, and if continued, may excite dysenteric inflammation of the intestinal mucous membrane. Hence, if pain with tenesmus and scanty mucous stools be observed, an attempt should be made to allay the irritation by opiate injections, and, if these fail, the use of mercury must be immediately abandoned. Counter-irritation may be employed more freely, and at an earlier period in these cases than in scrofulous affections of the hip-joint. When the inflammation has been somewhat subdued by local blood-letting, I generally employ, with advantage, a succession of blisters over the region of the hip. In the second stage, also, counter-irritation must be continued. Some surgeons prefer blisters, while others have recourse to caustic issues behind the trochanter major. When the disease is evidently confined to the osseous tissues, much benefit has been derived from cauterization by means of moxas or the actual cautery. Should any severe inflammatory symptoms appear, we must have recourse to antiphlogistic remedies, and administer purgatives, combined with saline and antimonial medicines. In the cases now alluded to, absolute rest may be enjoyed without any great danger of injury to the general health; and hence the patient should be confined to his couch, while the whole limb is kept immovable by a proper apparatus. With respect to other points of treatment, I do not know that anything of importance remains to be noticed. During the third stage of the disease, the same method is applicable to all classes of patients.—*Lancet, Dec.* 16, 1854, *p.* 499, *and Feb.* 10, 1855, *p.* 148.

50.—ON SINUSES OF THE HIP DEPENDING UPON EXFOLIATIONS FROM THE PELVIS.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[The patient who gave rise to the following remarks had come from Canada to be under the care of Mr. Syme, who commences his clinical lecture by a few introductory words which are important. He says,]

It is necessary that you should recollect the distinction between necrosis and caries. In the former disease a portion of bone dies, and separates from the living substance, so that no obstacle to recovery exists after the exfoliation or detached piece escapes from the position where it is situated; but in the latter the bone retains its vitality, and obstinately remains in a diseased condition, without any natural limit of duration, except the life of the patient or conversion of the caries into necrosis. It must further be recollected that the dense

osseous substance which composes the shafts of bones is chiefly liable to necrosis, and that the spongy or cancellated texture is almost exclusively the seat of caries. Now, sinuses about the pelvis are unhappily met with very frequently as the attendants or consequences of disease in the hip-joint, vertebræ, or sacrum, where the disease, being of an incurable kind, and the part concerned not admitting of removal, any sort of treatment can produce no better effect than a very imperfect degree of palliation, and it has hence been usual to regard such cases as of a very hopeless character. But nearly thirty years ago there happened to fall under my notice a case which showed that such a judgment should not be passed as a matter of course, or without more caution and discrimination than had been supposed requisite. The patient was a young man, aged 28, who, for the preceding seven years had suffered from sinuses about the hip and upper part of the thigh, which being regarded as proceeding from fistula in ano, had been so treated by the late Mr. George Bell and other surgeons, without obtaining the relief desired. He had then applied to quacks with no better success, and finally, abandoning all hope of recovery, had allowed the disease to pursue its course. It was a considerable time after this resolution that my assistance was asked. I found him extremely emaciated, and so weak that he could hardly leave his bed, with a large abscess of the thigh, and several sinuses about the hip, discharging matter profusely. Having opened the abscess, I examined the sinuses, and found that one, which opened in the fold between the buttock and thigh, led to the tuberosity of the ischium, in which there was a cavity containing an exfoliation of bone. I therefore dilated this sinus by incision, introduced my finger, and having found an opening between the origins of the extensor muscles, enlarged it by the bistoury, so as to obtain access to the interior, whence I removed a small bit of dead bone. The patient then quickly recovered, and ever afterwards enjoyed good health.

As there could be no doubt that the exfoliation had caused all the distressing symptoms of this severe and long-protracted case, it became very desirable to ascertain the origin of the evil, which could hardly be attributed to the ordinary influences of cold or external injury. Upon inquiry, it appeared that the patient had first experienced uneasiness after a day's employment in curing herrings, and that the discharge of this duty had required him to stand with his feet apart, alternately stooping and stretching his arms upwards to the full extent, subsequently to which he had felt a painful sense of fatigue in the back part of his thighs. It then occurred to me that the disease had originated from over-exertion of the muscles which arise from the tuberosity of the ischium, and that the bone with which they are there connected had suffered in consequence, so as to exfoliate, instead of being excited to preternatural growth, as Sir Astley Cooper, with apparently good reason, thought likely to happen, from inordinate muscular contraction. Several cases of a similar kind, which after-

wards occurred, tended to confirm this suspicion; and in a paper on the subject ('Edinburgh Medical and Surgical Journal' for 1828) I thus expressed my sentiments in regard to the pathological explanation:—"The history of these cases will, I hope, effect the great object of this paper, which is to excite a more discriminating diagnosis and active treatment of sinuses of the pelvis. As to the origin of the exfoliations, I will not at present say much. It seems very evident that they cannot result from the direct effects of violence, since, in all the cases detailed, the bone concerned was securely protected by its situation from any such injury. In all of them (if we except the first, where no information could be obtained as to the origin of the complaint), there was violent muscular contraction, and I am inclined to think that this may have been the exciting cause of inflammation and death of the bone. The subject is curious, and worthy of investigation, but of little importance when compared with the practical benefit which may result from a knowledge of the fact that sinuses of the pelvis sometimes depend upon loose exfoliations, which will not find their way out unassisted, but which may be readily removed artificially, with the effect of a speedy and permanent recovery."

We may now proceed to the case which has led to these remarks. The patient is T. C., aged twenty-four, a blacksmith in Toronto, Upper Canada. About a year and a half ago he was trying how far he could leap, when his right leg slipped as he came to the ground, and went out sideways from him. He felt shaken at the time, but did not experience actual pain until next morning, when his right thigh was so painful at its upper part that he could scarcely move: the pain was deeply seated, apparently in the very middle of the substance of the thigh, while the upper part of the adductor muscles was very tender to the touch. The pain continued for about two months, at first preventing him from working, and greatly crippling him, even to the end of this time. A lump then began to form at the extreme upper and inner part of thigh, a little anterior to the tuberosity of the ischium, and the pain gradually subsided. This swelling remained in nearly the same state for twelve months, when, having attained the size of a hen's egg, it was opened by a surgeon, and discharged about a teacupful of matter. Three weeks afterwards another abscess of larger size formed nearer to the groin, and was followed by several others of smaller extent, which were poulticed, and allowed to open spontaneously, while he lay in the hospital of Toronto, with apparently little prospect of recovery. Two medical gentlemen who had been educated at Edinburgh then became acquainted with the case, and having recognised its similarity to those pointed out by me as dependent upon exfoliation of the pelvic bones, advised the patient to seek my assistance. He accordingly travelled to New York, crossed the Atlantic, and was admitted to this hospital a week ago, (on the 30th Oct). The patient is now before you. You observe that he is a well-formed, healthy-looking man, and that there is no curvature of the spine, or

any other sign of disease, either there or in the hip-joint. There are several sinuses, one in the groin above Poupart's ligament, and others at the junction of the thigh with the perineum. Through one of the latter the probe passes towards the ascending ramus of the ischium, and there detects a loose piece of bone, which we shall now endeavour to remove.

[Chloroform having been administered, Mr. Syme dilated the sinus by incision, introduced his finger up to the origin of the adductor muscles, and having there felt a small aperture, enlarged it by means of a probe-pointed bistoury. He then extracted the exfoliation in three portions, which, when put together, fitted accurately, and constituted the inner margin of the pubic arch, the whole of which is concerned in affording attachment to the adductor muscles.]—*Lancet, Jan.* 6, 1855, *p.* 1.

On Amputation of the Thigh. By James Syme, Esq., F.R.S.E., &c.—Mr. Syme gives us the following hints on this subject. He says, "I have been long satisfied, that though in the upper part, or middle of the thigh, the flap operation answers well, yet, at the third, the circular, or a modification of it, is preferable; for, of course, the greater the distance from the origin of the muscles, the greater will be the amount of their contraction, and, therefore, the greater the necessity for guard as much as possible against its effects. Mr. Liston did not recommend operating lower down than the middle of the thigh on the ground that the stump obtained by operating in that situation was long enough to preserve the use of the hip-joint, and any more was superfluous. I cannot quite agree with this; and I suspect that he, while giving this advice, did so, though unconsciously, under the impression that better stumps could be formed in the upper than in the lower third of the thigh. In this boy's case, I shall operate immediately above the knee, by a modification of the circular method, which I have employed for some time past—viz., by forming two short flaps of skin by semilunar incisions on the anterior and posterior aspects, then retracting the skin so as to expose two inches of the muscles above the angle of union of the flaps, and dividing the muscles in front as high as they are exposed, and those behind as low as they are exposed to compensate for their greater contraction. The next point is one of great consequence—viz., to retract the muscles an inch and a-half or two inches before sawing the bone. The retraction of the muscles, previous to dividing the bone, is of great importance even in the flap operation, since it is not the length of the flap that prevents the risk of protrusion of the bone, but the height at which it is divided above the angle of union of the flaps. There is one advantage in operating low down in the thigh, which is perhaps of little importance here, where we have plenty of good assistants, but which is often of great moment in pri-

vate practice—viz., that a tourniquet can be applied without inconvenience, which is impossible when the operation is performed in the upper part of the limb."—*Lancet, March* 31, 1855, *p.* 333.

52.—ON AMPUTATION AT THE ANKLE.

By James Syme, Esq.

[The case which gave rise to the following clinical remarks of Mr. Syme, was that of a man, 24 years old, in a most pitiable state of suffering from disease of the tarsus. Mr. Syme decided on amputating at the ankle.]

The reasons why there is so little danger connected with it are, in the first place, that a very small portion of the body is removed; even part of the integument of the foot is left—viz., the skin of the heel, which forms the posterior flap, and the anterior flap, which extends a short distance over the dorsum of the foot; in fact, little more is taken away than in Chopart's operation; and the shock to the system, so far as it depends upon the extent of the part removed, must consequently be less than from amputation of the leg. Another consideration is, that there are no large bloodvessels divided in the operation—only the anterior tibial, where it is much diminished in size, and the plantar branches being concerned; there is therefore little blood lost at the operation, or risk of serious secondary hemorrhage. In the third place, the tibia and fibula are divided through the cancellated texture, and you thus avoid the danger connected with opening the medullary hollow, especially where there is a disposition to phlebitis, while at the same time, the spongy texture not being liable to exfoliation, like the dense part of the shaft, that inconvenience is prevented. On these grounds I anticipated that this operation would prove a very safe one; and this expectation has been now confirmed by long and very ample experience. Accordingly, I performed the operation in the present case, on the 13th of last month (February); and though I cannot undertake to say as yet that the patient is safe, yet he is certainly somewhat improved in general condition, and the stump is looking well. And, therefore, even if the patient should die, so far from this case leading you to think less favourably of amputation at the ankle, the improvement which has hitherto taken place will prove to you how very little disturbance of the constitution is produced by it. Patients, no doubt, may die after this amputation; but I believe they will hardly do so from the effects of the operation, if it be properly performed. In the first place, the incisions must be correctly made. A transverse one should be carried across the sole of the foot, from the tip of the external malleolus, or a little posterior to it, (rather nearer the posterior than the anterior margin of the bone,) to the opposite point on the inner side, which will be rather below the tip of the internal malleolus, but can be readily determined

by placing the thumb and finger at opposite sides of the heel. If the incision be carried further forward, considerable inconvenience is experienced from the greater length of the flap; and I believe a great deal of the difficulty that has been attributed to the operation has arisen from this source—the operator getting into the hollow of the os calcis, cuts and haggles in striving to clear the prominence of the bone, with the desperate energy of an unfortunate mariner embayed on a lee shore in a gale of wind. Another incision is then to be carried across the instep, joining the ends of the former. The next point to be attended to is, that in separating the flap of skin from the os calcis, you must cut parallel to the bone. This is of the greatest importance, since, when the flap is detached from the bone, its only supply of nourishment must be the branches which run through its parallel to the surface; and if, instead of keeping parallel to the surface you cut on the flap,—as a butcher does when he skins a sheep,—you will, by scoring it in this way, necessarily cut across these branches. I have reason to believe—nay, to know—that the sloughing which has occurred in some cases, has been due to these defects in the performance of the operation; the flap having been cut too long, difficulty has been experienced in separating it from the calcaneum, and this has led to the scoring of the flap, which has been inevitably followed by death of a portion, or the whole of it.

The dressing is the next point to which I wish to allude. It is of great moment, particularly in a case like this, where the tissues of the flap are very soft and weak, to avoid as much as possible the application of pressure,—tight bandaging would of itself be sufficient to kill the flap. I accordingly use a very light dressing for the first few days; and, in the present case, the flap was covered only with a piece of wet lint.

This is all I wish to say at present regarding the operation, but, before leaving this subject, I will make a few remarks upon certain alleged improvements which have been proposed. Some of these consist in differences with regard to the incisions, which have been made either diagonally, with the view of taking the flap chiefly from the inner side, or so as to split the flap into two lateral halves. Both these modes of operating are sometimes necessary from the circumstances of the case, and I have myself had occasion to resort to both; but I think that any one who has tried either of them as well as the ordinary method will give decided preference to the latter, wherever it is practicable; and I cannot help believing that the very gentlemen who have proposed the diagonal and median incisions would themselves have suggested the transverse had it not been before in use. Not that I wish to be understood to imply that the suggestion of these modifications arose from a desire on the part of those who made them to differ from other persons; far from it; but regarding the operation of amputation at the ankle as a good one, and honestly striving to improve it, they have brought themselves to believe that

the modifications above alluded to were really improvements. But the operation of a similar feeling would, I am convinced, have led much more readily to the suggestion of the transverse operation, had either of the others been that which was previously employed.

The next proposal to which I will allude is that of leaving the astragalus. My much respected friend, Dr. John Traill, of Arbroath, lately sent, through me, a communication on this subject, with an accompanying cast, to the Medico-Chirurgical Society of Edinburgh. Dr. Traill says very truly, that cases in which the astragalus could be left must very rarely occur, since disease affecting the tarsus at all generally involves the astragalus. But I think it would be very undesirable to leave this bone, even when it is not affected at the time of the operation, on account of its predisposition to caries; one of the great recommendations of amputation at the ankle being, that it removes all the bones which are liable to the disease. In Chopart's operation the calcaneum and astragalus are the only bones left, and though they appear perfectly sound at the time of the operation, yet the disease frequently recurs, as you may learn from the fact that I once performed three secondary operations, within the period of twelve months, upon stumps in which Chopart's operation had been practised. The bones I hold in my hand were removed in one of these operations; and you will observe that, as in all the other cases of the same kind, anchylosis has taken place between the astragalus and os calcis, while the latter bone has been previously drawn up by the action of the gastrocnemius, so as to prevent the patient from resting on the proper part of the stump. The same preparation shows the portions of the tibia and fibula that are removed in amputation at the ankle. They include, you see, the malleolar projections and a thin connecting slice of bone, merely sufficient to ensure removal of the articulating cartilage. In preserving the astragalus, therefore, you leave a bone which is peculiarly prone to disease, but there is another objection to this proposal. When you take away a foot, you must have in view the providing some substitute for it. It is true, the patient may walk without one, but practically it is necessary that between the leather of the boot that will be worn and the end of the stump a cushion of some soft elastic substance be interposed, and if the stump were as long as the sound limb it would not admit of this. Now, it appears that, when you amputate at the ankle in the ordinary way, the stump that results is just long enough to give the instrument-makers sufficient room for the material of the artificial foot; so that if the astragalus be left the stump will be too long. Also, if you retain the astragalus, the short, easily-formed flap of the ordinary operation will be insufficient to cover it, so that there will be danger of sloughing of the flap, as I have before explained. For these reasons, therefore, I consider the preservation of the astragalus as undesirable.

The third and last modification is one lately proposed by Professor

Peregoff, of St. Petersburgh, which on account of the distinguished source from which it emanated, has been tried in several quarters. The proposal is, to saw off the end of the os calcis, leaving it attached to the integument of the posterior flap, and then turn it forwards, so as to make it unite with the cut surface of the tibia. The objections to this method are, first, that the os calcis is a bone predisposed to caries, and it is therefore undesirable to leave any part of it; secondly, the operation is not facilitated by the change of procedure; but, on the contrary, considering the use of chain-saws, &c., involved, it is greatly complicated. Thirdly, the union of the osseous surfaces is not likely to take place easily. The fourth and principal objection is, that the integuments of the heel, which should form a cushion for the patient to rest on, are necessarily carried forwards, and the end of the stump is covered by skin, which is not designed for sustaining the weight of the body. I have, therefore, when consulted respecting the merits of this proposal, expressed a decided opinion against its adoption, and this advice has always been followed; but in some other cases, where it has been tried, the operators appear to have had reason for regretting that they did so.—*Lancet, March* 24, 1855, *p.* 307.

53.—ON AMPUTATION OF THE LEG.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[Mr. Syme prefers amputating the leg about an inch and a-half below the tuberosity of the tibia, where the bone is divided through the spongy texture, and the risk of exfoliation is avoided.]

It may here be proper to remark that I was at one period a very strong advocate for amputation by flap in this situation; but I have for many years adopted and recommended the method just mentioned. I feel no shame in making this confession; for it is no credit to any man to say that he never changes his opinions, which is as much as saying that he is incapable of improvement. If a man is continually varying his practice without good reason, he loses our confidence; but, on the other hand, as soon as a person is convinced of the superiority of any method of practice over that which he had previously advocated, he is not only entitled, but bound to adopt it. The arguments used in favour of the amputation by flap are, in the first place, that it can be performed with great rapidity; but this is now completely thrown overboard by chloroform, which makes it a matter of indifference whether the operation occupies half a minute or half an hour. In the second place, it is said that the thick muscular posterior flap provides a good cushion for the patient to rest on; and in operating on the dead body, the result of the flap operation looks very well; but in the living body the muscles are not passive and immovable, having, on the contrary, a great tendency to contract, and draw the flap away from the integument in front, and this, added to the weight of the

flap itself, produces a great risk of protrusion of the end of the tibia. Few persons can have practised the flap operation without experiencing this difficulty in a greater or less degree; and, as a strong illustration, I may mention that I have in this theatre within a period of six months performed three secondary amputations for bad stumps, in which large masses of muscle hung down, a mere incumbrance, while an incurable sore existed over the bone. But there is another remarkable circumstance. Even in the event of success after the flap operation, the cushion is useless, as the patient does not rest his weight on the extremity of the stump; and while the short stump is much the most convenient for the ordinary wooden leg, in which the patient rests his weight upon the bent knee, as in this apparatus, (this was the common pin leg), the curious fact is that the short stump is sufficient to allow the use of the knee-joint to be preserved. This was discovered by accident. A young man came here six years ago with the stump of an amputation performed at the middle of the leg, with a large ulcerated surface on its extremity, yet wearing an artificial limb, in which the use of the knee-joint was preserved, the stump being received into a cylindrical case. I remarked to him that he must suffer great pain in walking; he replied that this was not the case, and that he suffered pain only at night, from pressure of the bed clothes upon the ulcerated surface. I performed a second amputation in the way I have described, and the patient is now before you. You see that he sits differently from a person with an ordinary wooden leg, the pin not sticking out straight before him, but being in the same position as the sound leg. You also see that in walking he does not make the end of the pin describe an arc of a circle, but that the use of the knee-joint enables him to move it forward in a straight line like the foot.

[The patient, on being asked to state what he could do with his wooden leg, said that he had walked upwards of thirty miles in one day with it, and yet was able to walk well the next day; also that, being an engineer, he was in the habit of mounting to places, in the manufactory at which he was employed, of such difficult access, that few of the other workmen would venture to them; he then gave proof of his agility by running briskly up and down the stairs of the theatre.]

You see that the stump is as short as one in this situation could well be, the tibia having been divided only one inch below the tuberosity, the stump being formed by anterior and posterior flaps, and these, although now of considerable substance, were formed of integument alone, for I was obliged to remove as much muscle as possible, to admit of a covering for the bones after removing the end of the former stump.

Mr. Liston was at one time a strong advocate for a short stump in the leg, and he even went so far, in some of his operations, as to cut out the head of the fibula; but this is objectionable, because, with the head of the fibula, the attachment of the biceps is taken away, and

the stump is apt to become permanently extended, and there is also a risk of extension of inflammation of the bursæ to the knee. Before I performed the secondary amputation upon this man, I said to the gentlemen here that I had discovered that he did not rest his weight upon the surface of the stump, but on the patella, and that therefore the still shorter stump that would result from the secondary operation would probably be still sufficient to allow him the use of the knee-joint.

The apparatus which this man wears consists essentially of a case for the reception of the stump, made of *tin*, which he finds cooler than any other material, with a wooden pin adapted to its lower part, the whole bearing considerable resemblance to an inverted quart bottle without a bottom; a broad piece of thick leather laces firmly round the stump; and at its upper edge, on which the patella rests, is a leather collar, to be received on the upper margin of the tin case; the leather case is fixed by a strap at each side to another broad piece of leather, which laces round the thigh just above the knee, and the whole apparatus is thus kept securely in position. It is a remarkable fact, showing the slowness with which improvements are received, that all those persons whom you see walking about so laboriously to themselves, and so inconveniently to their neighbours after amputation of the leg, might walk like the man before you by the use of a similar apparatus.

The operation, as I performed it the other night, is as follows:—Take the tuberosity of the tibia, or the head of the fibula, as your guide, and make a short anterior and posterior flap of integument only, entering your knife about a hand's breadth below the bony prominence, at the point which is to form the angle of junction of the flaps at one side. Having dissected up the integuments to an inch and a half below the tuberosity of the tibia, divide the muscles behind about an inch lower down, to allow for their retraction, and then saw through the bones as high as the skin has been dissected up. The stump thus formed is, as you have seen, long enough to preserve the use of the knee-joint. The medullary cavity is not opened, nor do you cut through the dense bone that is liable to exfoliation, and the wound that results is small, and generally heals rapidly.—*Lancet, Feb.* 24, 1855, *p.* 203.

54.—*Diagnosis of Fracture.*—[Most surgeons dislike fractures of the long bones in children, especially when near the joints. The ends of these bones are more vascular and more disposed to ulceration than the middle parts. Mr. Stanley makes the following practical remarks on this subject:]

I think 'Pott's fracture,' (said Mr. Stanley, much to the amusement of the class,) is the only one well understood by candidates coming up to the College. Those industrious gentlemen, the grinders, make them

have Pott's fracture quite pat. Now, every broken leg is not Pott' fracture; it is, in fact, a rather insignificant accident, fracture of the lower third of the fibula, with dislocation of the tibia and eversion of the foot; it is rather a dislocation of a bad and troublesome kind, first explained very admirably by Mr. Pott, than an injury of such startling and very momentous importance as generally believed. A man jumps from a height, the foot twists outwardly, the foot is drawn up by the peronæi, and you have an injury easily recognised; but take care, when you are called to what may or may not be fracture of the upper end of the same bone and tibia, near the knee, with hemorrhage into the cancellous structure of the bone, or fracture without crepitus of the lower end of the femur, or what is nearly the same. swelling of the elbow, and a fracture not to be made out in the humerus,—in all these cases, I would advise you to act with great caution, and as if there were fracture really present. You will have, in fact, hemorrhage of the bone, and effusion into the joint, which, I would repeat again, is one of the most untoward complications of these injuries, leading, possibly, very often to false joints or ununited fractures, and reflecting no very conspicuous credit on whoever has the fortune to meet with such cases.—*Lancet, Dec.* 16, 1854, *p.* 506.

55.—ON FRACTURE OF THE THIGH.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[Mr. Syme thinks that too much attention is paid to attempts to overcome muscular contraction by extension, instead of preventing it by careful adjustment of the broken bones, and equable support of the limb.]

In children, fracture is usually in the upper third or the middle; in adults, commonly in the lower third; in old persons, generally in the upper part, through the trochanter or neck of the bone; but at all ages the treatment is extremely simple and uniform. A hair mattress is much preferable to a feather bed, which is so unequal and yielding, that I would rather treat a patient with a fractured thigh upon straw. Draw out the limb to its full length, which you ascertain by comparing the distance between the knee and spine of the ilium with that on the sound side; feel that the fragments are, so far as you can judge, in proper position; then apply at each side of the thigh a splint of wood, pasteboard, or leather, extending on the outer side from the knee to the great trochanter, and on the inner side from the pubis to the inner condyle. It is of little consequence which materials be used; wooden splints such as these (Gooch's) are very convenient for private practice, as they can be easily cut down with a penknife, and at once adapted to a limb of any size, from a thigh to a finger; pasteboard and leather, however, are more accommodating, and can be fitted with greater accuracy to the limb. The splints are retained in position by

bandages, which should not be rolled continuously round the limb, but applied at intervals, in the following way: a piece, rather longer than twice the circumference of the limb, is doubled, and passed round; one of the ends is put through the loop, and tied in a bow to the other end. Three or four such "looped bandages" are applied along the thigh,—three being sufficient, if the splints be of wood, while an additional one is desirable with leather or pasteboard, on account of their softness at the time of application. The advantage of this method of bandaging is, that the state of the limb can be observed for the most part without disturbing the bandage; and if it becomes necessary to make a fuller examination, all that is requisite is to untie the bows, when the splints can be separated from the thigh without its being raised from the bed. The spasmodic action of the muscles is continued, in consequence of their being pricked by the sharp ends of the broken bones. Now, when these lateral splints have been applied, although the muscles are compressed, and the risk of displacement diminished, yet whenever the patient moves a joint of the affected limb he moves the broken bone, and it is therefore necessary to restrain the motions of the whole limb; hence the great advantage of Desault's splint, which, though powerless as a means of overcoming muscular contraction, is of inestimable value in maintaining the quiet of the limb; the patient cannot move his toes without moving the ankle also, nor the ankle without the knee, nor the knee without the hip-joint, so that if he moves his great toe there is a tendency to displacement of the fragments of the femur. It is curious that Desault has given a wrong representation of his own splint, and while describing it correctly, as extending from the false ribs to a little below the foot, has figured it as reaching up only to the great trochanter. Unless it extend to the false ribs, it has no power in restraining the motion of the hip-joint. In ordinary practice, the long splint is generally not made sufficiently broad; it ought to be equal in breadth to the diameter of the limb, or about four inches and a half for an adult; otherwise there is great difficulty in preventing eversion of the foot. The best means of fixing the limb to the splint is a sheet wrapped round the splint till a part equal in breadth to the circumference of the thigh remains unrolled; this is passed under the leg, and wrapped round it; after which its free margin is secured by strong pins to the part rolled round the splint, which serves also as a pad to the limb; it is also well to use a perineal band, not to produce extension so much as to act as an additional means of insuring the perfect rest of the limb; the foot being fixed to the lower end of the splint by a handkerchief passed round the ankle just above the heel, and crossed over the dorsum of the foot, the ends being passed through the holes or notches in the splint, and tied upon its outer surface; a broad bandage carried round the chest and upper part of the splint completes the apparatus. The sheet is far simpler and more effectual than rollers applied from the foot to the groin; two or three bandages, six yards in length, are required for that

purpose, and though they look very neat when first applied, they soon require re-application; the state of the limb can never be ascertained without either raising the thigh to take off the bandage, or cutting it up, and in the latter case the thigh must be raised in order to re-apply the bandage; whereas, when the sheet is used, on taking out the pins, and throwing the sheet aside, the limb is exposed undisturbed for examination. This apparatus has also the great advantage that it can be obtained extemporaneously; there are few patients, rich or poor, whose houses do not contain a piece of wood that would answer for a long splint, and a sheet or tablecloth to wrap round it.

I consider that it is a great error to teach surgery only adapted to hospitals. Complicated apparatus is, in my opinion, not so good as the simple; but even if the complicated means were the more effectual, the other ought also to be taught. But I would trust to the simple apparatus before you, rather than to the most complicated ever devised.

Some surgeons who would go along with me thus far, make an exception in the case of fracture of the thigh in children, saying, that the fracture being in the upper part of the thigh, the upper fragment must be tilted upwards by the action of the psoas and iliacus, and that therefore the double inclined plane is preferable to the long splint for children. This, however, is a merely theoretical objection, and ample experience enables me to testify that the long splint is as applicable for children as for adults. I admit rare exceptional cases, however; for instance, I have found it necessary to use the double inclined plane when the knee has been anchylosed in the bent position, and again in one case where the leg had been amputated. Other cases also do occur, though very rarely. A man was once brought to this hospital with a simple comminuted fracture of the thigh, for which the long splint was applied; the patient, however, obtained no relief from it, but suffered such extreme agony, that it made him even get out of bed and drag the splint after it. I confess he met with but little sympathy, being supposed to be affected with delirium tremens. After a while swelling of the limb came on, followed by ulceration, and protrusion of the bone in the ham, and I was obliged to amputate the thigh. On examination of the parts, we found that the fragments, instead of forming as usual a convexity forwards, projected backwards, and one fragment as sharp as a needle had penetrated the popliteal nerve, and obliterated the vein, so that the extreme pain, and the swelling of the limb were both accounted for. It happened that a patient came in soon after with fracture of the thigh, with the fragments in the same position. My late house-surgeon, Mr. Bickersteth, told me that he could not make the bones lie straight with the long splint. We therefore used the double inclined plane with a perfectly successful result.—*Lancet, Feb.* 17, 1855, *p.* 173.

56.—*The Starched Apparatus in Fractures.* By Chas. Royston, Esq., Liverpool.—[In support of Mr. J. Z. Lawrence's practice, Mr. Royston brings forward the practice adopted in Liverpool, in the Southern and Toxteth Hospital. He says,]—We apply a dry bandage next the skin; then two pasteboard (side) splints, made perfectly soft by soaking in hot water, and moulded carefully to the shape of the limb; and, lastly, a bandage which has been tightly rolled in a thick solution of starch. This becomes thoroughly dry and hard in about twelve hours, forming a very firm case for the limb. The patient is allowed to walk about on the following day, having his foot suspended on a sling, reaching from the neck to the foot, and being provided with a pair of crutches. The only cases in which we have any fear of employing it are young children of a strumous habit of body, whose skin is generally tender and irritable. In such cases we should immediately remove it if the little patient complained much of pain.

We often employ it in fractures of the shaft of the humerus, occasionally when the elbow-joint is implicated, and in one case of fractured olecranon it was used with very good result. It has been used here in fractures of the lower end of the radius, when there has been no great displacement, with success.

In fracture of the femur, the long splint is always kept on for a month prior to applying the starched apparatus, and the long splint (without the pad) for a day or two afterwards, until the apparatus has become thoroughly dry and firm.

Stiffness of the knee-joint often happens after fracture of the femur, if the joint has been included in the starched apparatus; we only carry the pasteboard splints as low as the condyles of the femur, and do not include the joint.

In oblique fractures of the leg, we sometimes use M'Intyre's splint for ten days or a fortnight, and then put the case up in starch.

The starched apparatus has been found to answer very well in fracture of the leg immediately above the ankle-joint.

I treated one case of Pott's fracture of the leg with it last year. The patient (a female) went out at her own request at the expiration of a fortnight, and ten days after the starch apparatus had been applied. She attended at the hospital at the end of the month, when the apparatus was removed. The bones of the leg were firmly united, and there was considerable freedom in the use of the ankle-joint.—*Med. Times and Gazette, Dec.* 16, 1854, *p.* 623.

57.—ON SUBCUTANEOUS OSTEOTOMY.

By Dr. P. Frank.

[Various have been the methods of attempting to rectify deformed bones.]

In the hands of Dupuytren, Gräfe, Dieffenbach, and Langenbeck, forced infraction of the callus has in numerous cases been attended

with great success. Powerful extension and counter-extension being maintained, pressure was performed against the seat of fracture, either by manual force or by use of pulleys. In other cases, direct pressure was resorted to, either against the convexity or the concavity of the curve. The latter practice was particularly recommended by Dieffenbach, who fractured the bone in the direction of the concavity, over a wooden block, hollowed and well padded on its superior aspect. In order to fracture the deformed bone in the opposite direction, namely, towards the convexity, the machine of Booth, and the formidable dysmorphosteopalinilastes of Osterlen, were invented. As above mentioned, success has often attended operations performed according to the methods described; but there is no lack of unsuccessful and even of fatal ones. It is obvious that, on account of the great density and firmness of callus, the bone can easily be fractured above or below the part designated, and that in cases where the fracture is produced in the right place, great apprehension must be entertained of laceration of muscles and integuments ensuing by the sudden and violent action with which the fragments are thrust against these parts.

If these operations are carefully conducted, these evil consequences will, in a majority of cases, be avoided; and if a moderate force is likely to suffice to produce infraction of the callus, they will still form the best resource of the surgeon, and deserve a trial before other measures are resorted to.

Where the methods above described should not succeed, or, *à priori*, are not deemed practicable, as in cases where the callus is very voluminous, and perfectly consolidated, or in cases of rachitic deformity where the sclerotic state of the bone is fairly developed, osteotomy alone can promise success. This was first performed by Wasserfuler, who, in a case of fracture four inches below the great trochanter, which had been allowed to consolidate with great deformity, cut down upon the angle of the bone, divided two-thirds of its whole diameter with the saw, and fractured the remaining portion over a pyramidal block.

In the year, 1852, Mayer, of Wurzburg, published an account of thirteen cases of various osseous deformities successfully treated by osteotomy, a word which was introduced by him into surgical nomenclature. Mayer cut down upon the bone, and, varying his practice according to the nature of the deformity, performed either transverse, oblique, and semi-circular incisions through, or cuneiform excision out of the bone. The large wound in the soft parts, the free exposure of the bone necessary for the execution of this operation, the violent reaction resulting after it, render its performance too dangerous to admit of general adoption. These objections to the practice of osteotomy are successfully obviated by a new method, for which operative surgery is indebted to the inventive genius of Professor LANGENBECK, of Berlin.

It would be impossible to give a more lucid and condensed account of this brilliant acquisition of modern Surgery than by offering an uncurtailed translation of the paper published by Professor Langenbeck a few months ago.

I have lately seen the cases of Professor Langenbeck, and have assisted at an operation of the same kind, successfully performed by Dr. Wagner, of the Dantzig Hospital. The details of this case shall be given as an appendix to those operated upon by Professor Langenbeck himself.

"After the introduction of the narrow straight saw for the resection of bone, during the Schleswig campaign of 1848, the idea of performing subcutaneous resection could not fail to arise. In fact, several operations were made in course of this campaign which well-nigh deserved that name. I allude to the cases where I excised fragments of the humerus, exposed by suppuration, by aid of the narrow saw, without any further lesion of the soft parts than a slight dilatation of the gun-shot wound.

"At that period I had already determined to perform subcutaneous re-section in other cases, and have since then frequently expounded the principles which were to direct its execution. This was particularly done in speaking about the case of ununited fracture of femur, where I cut the fragments with the narrow saw, and in several clinical lectures in the years 1851 and 1852, after failing to succeed in the forced rupture of two cases of osseous anchylosis.

"The patients were in both cases young and healthy individuals; the anchylosis was in one case the result of a penetrating wound of the knee-joint with consecutive suppuration; in the other of acute rheumatic disease of the same articulation. In both cases there was probably extensive osseous coalescence of the articular surfaces; and that of such firmness, that the employment of greater force would undoubtedly have fractured the condyles of the femur.

"The excision of a cuneiform piece out of the anchylosed joint in imitation of Barton, now appeared to be the only plan which promised success; but considering this operation highly dangerous, on account of the extensive osseous wound, I proposed subcutaneous osteotomy in its stead. It was my intention to introduce a very narrow straight saw into a hole drilled through the anchylosed joint, and then to divide the bone so far, that a moderate force would afterwards suffice to straighten the limb. Both patients were ready to submit to the operation, if I would assure them that no danger would result from its execution. This I was not able to do, as osteophlebitis may be the consequence of any operation on bone, and both patients left the hospital. Another case of anchylosis where forced rupture proved unsuccessful, has since then not occurred; but in February, 1854, a case of curved rachitic tibia presented itself, in which the state of bone was such that forced infraction was utterly impracticable.

"I therefore determined without hesitation to perform subcu-

taneous osteotomy, but chose to defer its execution to a warmer season, erysipelas and pyæmia being at that time particularly prevalent.

"During the summer term of the present year, I have already performed subcutaneous osteotomy in three cases, and that with results so brilliant and encouraging that I believe myself justified in entertaining hopes that this new operation will supply a desideratum till now often experienced.

"The instrumental apparatus I made use of consists in—

"1. A fluted chisel-drill, two lines in breadth, attached to a centre-bit.

"2. A straight saw, 1½ lines in breadth.

"3. A common strong scalpel.

"The operation having till now only been performed on the tibia, the execution was in all cases much the same.

"An incision, rectangular with the longitudinal axis of the bone, was made on the internal surface of the tibia, dividing the integument and periosteum at once.

"I then applied the drill in the centre of this incision, and perforated the bone transversely from within outwards. The cessation of resistance safely denotes the final accomplishment of the perforation, so that no danger of injuring the anterior tibial nerve and artery need be apprehended.

"The narrow saw is now introduced into the aperture drilled through the bone, and the greater part of the latter divided, by sawing, both in the direction of the crista or towards the posterior surface of tibia, without any further enlargement of the wound in the soft parts.

"The bone is directly straightened by fracture of the remaining osseous bridges, or this proceeding can be deferred till the consecutive inflammation and suppuration have subsided. In the first two cases, I followed the first plan, indulging the hope that the wound of the integuments would heal without suppuration; but, after having experienced that this could not be expected, I performed instantaneous fracture in the third case.

"Further observation will have to decide on the comparative merits of these proceedings; at present I am inclined to far prefer the latter.

"Two reasons have induced me to divide only a part of the bone by the saw; the first is the great difficulty which the small wound of the integuments would oppose to perfect division; the second and more important one is, that the great mobility of the fragments necessarily resulting after perfect division by the saw, is avoided by consecutive gradual fracture, which will always allow some osseous bridges to remain intact, rendering the fracture imperfect.

"Experiments and future experience will yet have to show the extent of the subcutaneous division of the bone necessary to permit of easy fracture of the remaining parts.

"In the third case which I operated upon, this act was attended with great difficulty, a powerful effort being required to fracture the hard and voluminous callus.

"This procedure will no doubt be rendered easier of execution, if the bone is perfectly divided from the centre towards the concavity of the curve, instead of allowing bridges to remain on both sides, as I have done, till now.

"As to the local and constitutional effects of the operation, they are so insignificant, that subcutaneous osteotomy cannot be compared with a resection of bone, exposed by a large external wound. The pain experienced directly after the operation, and the febrile re-action, are scarcely more considerable than after simple fracture.

"Three or four days after the operation the soft parts around the wound are slightly inflamed and painful on pressure.

"Closure of the wound by first intention happened in none of the cases operated upon, suppuration supervening from the sawn surfaces of the bone.

"In the second case the discharge was not inconsiderable, and a fortnight elapsed before it subsided.

"Suppuration ceases as soon as the drilled canal and the saw-wound are filled with granulations; the external wound is then rapidly closed, and the bone is in the same condition as in consolidatory simple fracture.

"I regard it as no inconsiderable advantage connected with this operation, that, whether it be performed to straighten a bone after falsely united fracture, or a curvature of the bone after disease, or an osseous anchylosis, the normal position need not be perfectly restored at once, but can be gradually effected during the consolidation of the bone; thus rendering it possible to avoid any undue contusion and laceration of the soft parts.

"From the experience till now collected on the operation, I think myself justified in drawing the following inferences:—

"1. Bone can be subcutaneously divided, like tendons and muscles, and subcutaneous osteotomy bears well-nigh the same relation to resection with extensive division of the soft parts, as subcutaneous tenotomy to division of the tendons after dividing the integuments.

"2. Partial subcutaneous division of the bone by the saw is preferble, because perfect mobility of the fragments is thereby avoided.

"Union, by immediate formation of callus, as in simple fracture, cannot be expected after subcutaneous osteotomy, because the small particles of bones detached by drill and saw act as foreign bodies, which must be eliminated by suppuration."—*Med. Times and Gazette, Dec.* 16, 1854, *p.* 612.

58.—ON DISEASES OF THE BONES.

By John Bishop, Esq., F.R.S., &c.

[In this paper, Mr. Bishop enters on the subject of *exostosis,* such tumours as are situated immediately under the periosteum, and are at first disconnected with the bone, but nevertheless, after having been formed for some time, become sooner or later firmly united with the contiguous bone. They are liable to form on the internal table of the bones of the head.]

A tumour of this kind presents to the eye the appearance, and to the touch the form, of a nodule, with an abrupt margin, and is frequently separated at its base from the subjacent bone, by a furrow of variable thickness; and sometimes there is a deep fissure between the nodule and the bone lying beneath. The density of these compact nodules is very great, and in this state the disease is termed the ivory exostosis. These ivory tumours are of greater density and specific gravity than the normal bone with which they are associated, unless they happen to be connected with bone already in a state of induration. They are always composed of laminæ, and seem never to be intermixed with spongy tissue. The density of the tumour is owing to the greater number of laminæ contained in a given space, when compared with the number of laminæ found in the same space in normal bone. The Haversian canals are small, and few in number, but a well-defined lamellar system is found surrounding them. The bone corpuscles are irregularly scattered in the substance of the tumour, and in some places they are clustered together, while in other parts larger tracts are found entirely destitute of them. The colour of these exostoses is of a yellow-white, and they are of a lighter hue than the bone to which they are attached.

Rokitansky appears opposed to the hypothesis that the origin of these tumours has any relation to syphilis, and he believes the cause of their appearance to be entirely unknown. When the exostoses are of a spongy texture, they are connected with a rarefaction or expansion of the spongy bones, termed by the German pathologists *osteoporosis.* They present many varieties in their structure, and may arise either from the spongy or from the laminated structure, or from both together; they are then compounded of an internal spongy, and an external laminated layer of the compact tissue. These mixed osseous tumours are not unfrequently found near the joints, at the head of the tibia, or on the shin, and on any of the other long bones of the skeleton. In the skull, the disease is accompanied with an expansion of the diploe, in which case there may be an exostosis on both the external and internal tables corresponding in situation with each other. In the spongy forms of exostosis, the structure may remain permanent after its development; but more commonly new matter is formed in the interior, and the structure of the bone is more or less condensed; but this altered structure is often succeeded by the

bone being again rarefied, and thus the growth of the spongy exostosis outwards may be affected, and may increase to a considerable size. When this disease affects the spongy bones of the face, it has been known to produce the most hideous appearance. Some examples of these cases are to be seen in the Hunterian Museum. When the exostoses have acquired a certain magnitude, they usually continue through life unchanged. The ivory texture of the excrescence, in this disease, sometimes diminishes in size by condensation, or it may become necrosed, and thrown off.

The spongy exostoses sometimes degenerate into caries, and in this way disappear. These appear to be the processes by which the system may be spontaneously released from the osseous growths of exostosis. Independently of exostosis, the bones during inflammation exude a fluid which ossifies, and forms a layer on their compact surface, this substance has received the name of *osteophyte*. At the commencement of this process the exudation is soft and gelatinous; it afterwards becomes tough and elastic, resembling cartilage, and in the end it ossifies. It appears that this exudation is associated with nearly all inflammatory conditions, whether of abscess or necrosis of the bones. There are many variations of form assumed by these osteophytes, as well as differences in structure.

They sometimes appear to be composed of "delicate fibrils and lamellæ, which are fixed at acute angles on the surface of the bone, and give it the appearance of velvet, or felt with a very fine nap." Though these osteophytes are at first in contact with the bone, yet they may be easily raised from it in large pieces, and although for some time separated by a layer of cellular substance, they become fixed to the bone when this membrane disappears.

The exudations which terminate in osteophytes sometimes form plates, and often anchylose some of the vertebræ, by the ossification of the anterior common ligament. They occur also on the inner table of the skull, and, indeed, wherever inflammatory processes are going on in the bones.

Osteophytes have been known in a few cases to cover large portions, or nearly the whole of the skeleton; they are sometimes associated with atrophy, and at others with hypertrophy, of the bones. Although we generally find the exudations accompanied with an inflammatory condition of the bones and periosteum, the precise condition of the system that gives rise to the inflammation and exudation is not yet determined. In the imperfect union of fracture, these plates then constitute the union of the ends of the bone. To give a minute detail of the structure of all the forms of osteophytes would occupy too much of your attention in this short course, I have therefore merely touched on the subject to show the distinction in their character which has led to their separation from exostosis.

The bones of the skeleton are likewise liable to anomalies in number, size, and form. The absence or addition to the number is not

very common, but is coincident with conditions of the system in a state of perfect health, and not referable to any pathological conditions: it is not, therefore, my purpose to give any detail of cases of variations of number. With regard to variations in size depending on pathological states of the bones, the case is quite different, and deserves a few remarks. In hypertrophy of the bones, they may increase in size by the addition of new osseous matter deposited on their surface beneath the periosteum, without any change taking place in their density or specific gravity; the size of the medullary canal remains the same, but the compact substance acquires a considerable augmentation of thickness. When the increase of substance takes place in the interior of the bone proceeding from the Haversian canals, and involving the whole of the medullary system, the bone becomes more dense, both in the compact and cancellous tissues. In the latter, the walls of the cells increase in thickness, and the medullary cavity diminishes in size; the diploe disappears, and the whole bone becomes indurated. When the hypertrophy takes place both internally and externally, the bulk and density of the bone are both increased. The increase of the bones, both internally and externally, is preceded by the deposition of a superabundance of cartilaginous matter, in which the salts of lime are deposited, as in normal bone.

When the disease attacks the bones of the skull, or when it affects considerable portions of the skeleton, it becomes a serious disorder. It is a remarkable circumstance, that when the bones of the skull become hypertrophied, those of the face often diminish, so that there are actions opposite in their effects—namely, hypertrophy and atrophy, going on in the system at the same time.

The conditions of the system that give rise to these changes in the bone are not yet determined, nor do the bones themselves give any promonitory signs of the advent of the disease. The surface of the bone continues smooth, and the periosteum natural, even when the bone has acquired the compactness of ivory. In other cases, the increase in the volume of the bone is preceded by an inflammatory condition. The seat of the inflammation may be either the periosteum and the compact tissue, or the medullary membrane. When the external surface is affected, an exudation takes place on the surface of the bone, which becomes ossified into laminæ: this forms a layer, which is sometimes separated from the surface of the bone by a layer of spongy tissue. When the inflammatory process has its seat in the medullary membrane, it leads to increased density either of the compact or of the spongy tissue, or of both these structures. It may be easily imagined that these organic changes cannot take place without affecting the texture of the diseased bone; and it is found accordingly, that the substance of the bone becomes hypertrophied in consequence of the expansion and infiltration of the tissue connecting the capillaries of the medullary canals and cells. The result of these organic changes is a thickening at the affected portions, the other sections of the bone re-

maining free, so that the surface often presents a rough and uneven appearance. The inflammatory state of the bones under consideration are frequently experienced by persons labouring under a rheumatic, syphilitic, or gouty constitution, and the treatment must depend on the nature of the causes that are associated in the production of the disease; the local treatment is that common to other organs affected with chronic inflammation.

The opposite state to that of hypertrophy, is atrophy of the bones. Whatever causes lead to the undue nutrition of bone, tend at the same time to diminish its volume. There are, consequently, many states of the system which may lead to atrophy, such as indifferent diet, want of action, exhausting diseases, palsies, fractures, anchylosis, and, indeed, anything which tends to impede the flow of blood to the bones, are all accompanied with atrophy. From the nature of the various causes just mentioned, any of the bones of the skeleton may be affected. In many cases, the bones diminish both in length and thickness, and the medullary canal becomes contracted; this condition of the bone is termed *concentric atrophy.*

Independently of special causes, the bones of aged persons often become atrophied; they are then brittle, and break with a comparatively slight force. It appears that atrophy always begins in the medullary canals and in the diploetic structure, the cells of which enlarge, and the walls and lamellæ of the cancellous structure become attenuated, and finally disappear. The compact tissue becomes changed, and resembles the spongy diploetic structure; and the outer layer only remains unaffected, but almost as thin as paper. As the atrophy of the spongy substance advances, the external layer only remains, and incloses a cavity, with mere traces of spongy tissue at its periphery, or a soft substance with large cells. When the spongy substance is entirely removed, the thin external walls of the bones approach each other, and form a single plate. When the cavity within the bone is enlarged, it is called by Mr. Curling *ex-centric atrophy.*

The con-centric form of atrophy occurs in the larger medullary canals, the ex-centric in the bones of the pelvis, ribs, and vertebræ. It will be easily imagined, that when such great organic changes take place in the bones, their physical characters alter in proportion; and accordingly, the bones thus affected become flexible, and crack when they are bent. As the loss of the internal portion of the bone proceeds, the external portion becomes diminished, and hence the skeleton in *senile* atrophy, together with the whole weight of the body, is less than in the normal state. The volume of the bones may be also diminished by continual pressure, such as that produced by tumours and aneurisms; but to these purely mechanical causes I do not wish to engage your attention.

The following proportions were found to exist between the inorganic and organic matter, in a healthy, thickened portion,

and in an exostosis formed on the same bone, analyzed by Lassaigne :—

	Healthy.	Hypertrophy.	Exostosis.
Inorganic	57·	58·4	54·
Organic	43·	41·6	46·

Having now given a description of the nature and growth of osseous tumours, I shall occupy a few spare minutes of your time with some very brief remarks on the nature and diagnosis of the soft tumours affecting the bones. Perhaps there is no subject in which the microscope has been of greater utility to the practical surgeon, than in determining the structure of the soft tumours of the osseous system. Up to a very recent period, tumours of the bones of the most heterogeneous and diversified character have been assembled together, and no distinction has been made between those of the mildest and those of the most malignant tendency. Thus, the older writers grouped together, under the term "*osteo-sarcoma*," the cartilaginous and osseous, the osteoid, the myeloid, and the enchondromatous tumours, and confounded these non-malignant with the scirrhous, medullary, and areolar tumours. Under these circumstances, we cannot wonder that much discussion should arise amongst surgeons on the propriety of, and the varied success resulting from, the removal of these mixed forms of tumours.

When we look into authors on osteo-sarcoma, we find that they had no very precise idea of the nature of the tumours comprised under this term. Some wished to restrict the term to those which are decidedly scirrhous, whilst others describe them to be of a cartilaginous texture, and none seem to have satisfactorily determined whether they are or are not wholly malignant. With such a state of confusion and complication, we need not be surprised that some operations have been attended with complete success, while in other cases the diseases have returned and destroyed the patient. To give an idea of the complete difference in the views entertained by surgeons on this subject, it is only necessary to refer to one or two of those who have confounded together the malignant tumours with those of the non-malignant character.

Thus, according to Callisen, osteo-sarcoma is a disease by which the texture of the bones is converted into a lardaceous substance, having a tendency to carcinoma. Boyer considers it a disease analogous to cancer. Dr. Cuming, of Glasgow, considers that though all varieties of osteo-sarcomatous tumours are highly formidable, yet he says "they are not all really cancerous." Mr. Mayo observed of these tumours, that they have not "much malignity;" so that if all the portion of the bone involved, with part of the adjacent sound bone, be removed by amputation, the complaint seldom reappears, either in the part, or in another bone. It would be an almost endless task to enumerate the different opinions, with the various results of these

tumours; but we see how little dependence can be placed on the results respecting the cases that have been detailed, unless we could be secure of the real nature of the tumour removed. But the diagnosis of the character of the tumour is in some cases by no means easily made, and I have occasionally been in consultation with the most distinguished surgeons of the metropolis without arriving at anything like a satisfactory result.

The diagnosis of the nature of many tumours connected with the bones is one of the problems in surgery most difficult to solve; the data in many cases are not sufficient for the purpose. The hereditary tendency to diseases of an organic type, the aspect of the patient, the situation of the tumour, its character to the touch, and the history of its formation, are all circumstances to aid in the research; but all these are often insufficient to enable the surgeon to arrive at more than an hypothetical conclusion. An example will suffice to show the truth of these remarks.

A few years ago I was solicited by a gentleman to remove a tumour situated at the back of the thigh. On examination, I found it was deep-seated, soft to the touch, and apparently moveable on the bone; it was of a considerable size, and had only been recently detected by the patient. Having some suspicion of its character, I advised his having another opinion before its removal, and Mr. Guthrie was consulted; but that gentleman being of the same opinion as myself —namely, that it presented a formidable aspect, it was agreed to take the opinion of the late Sir Astley Cooper. In this consultation, Sir Astley stated, that although he was of opinion it was very likely to turn out malignant, yet that its removal would give the patient a greater chance of a prolonged life. However, considerable alarm having been excited in the minds of the patient and his family by these consultations, he was advised to take the opinion of Mr. Lawrence, and subsequently that of the late Mr. Earle. The result of their several opinions was, that it was a tumour of an uncertain character, but all agreed on the propriety of its removal.

At length the family decided that Mr. Earle should be selected to perform the operation. On cutting down on the tumour, it was found to be a medullary cancer connected with the bone, with ramifications so extensive that its complete removal could not be accomplished. The consequence was that the wound did not heal, and the patient sunk from exhaustion at the end of about three months. In this case it will be observed some of the best surgeons in London were consulted, notwithstanding which nothing but an uncertain knowledge could be formed of the character and connexions of the tumour, and it was, as the result proved, a case which required either amputation at the hip-joint, or to be left unmolested.

In order to assist us in forming a correct diagnosis, of the nature of tumours, it has been suggested, and attempts have been made, to ascertain their character by means of a grooved or hooked probe,

but with what success this plan has been attended I am not prepared to state; still it seems to hold out a feasible prospect of enabling us to form a better opinion of such cases before operating for their removal. It is the opinion of many surgeons that malignant diseases in the bones are not so liable to return after operation, as those of the soft parts; but as these opinions were given when the real nature of tumours was less understood, it will require further investigation to determine this question. Myeloid and enchondromatous tumours were, until very lately, considered malignant; and this may have given rise to many of the opinions formed of the curability of the patient suffering under malignant tumours of the bones by means of their removal.—*Lancet, Dec.* 16, 1854, *p.* 500.

59.—CASE OF MALIGNANT DISEASE OF THE ILIUM.

By W. Fergusson, Esq., F.R.S., Surgeon to King's College Hospital.

[The following interesting case gave rise to some valuable clinical remarks by Mr. Fergusson.]

T. B., aged 56, a bookseller at Birmingham; states that he has always been very healthy, although subject latterly to great debility. About last Christmas—that is, nearly four months ago—he began to notice a pain in the posterior part of the left haunch bone, which was at first trifling, though increased by the slightest exercise, and always relieved by rest. The pain gradually increased in severity until six weeks since, when it came on rather more sharply than usual, and then it was that he noticed a swelling in the situation of the pain. This swelling was then as large as the top of his thumb; and the pain, when it commenced, was, according to his feelings, situated in it. He moreover states, that, when he first discovered the tumour, he found it pulsate distinctly. From its appearance to the present time the tumour has been steadily and slowly increasing. Present condition:—The patient is in a state of great debility, which has been slowly coming on for the last two years. Bowels much confined; appetite good; sleeps well; pulse 90 and weak. There is a tumour situated on the posterior part of the left dorsum ilii, above and slightly over the great sacro-sciatic foramen. The crest of the ilium can be felt distinctly above it. The dimensions are, five inches in one direction, and four in the opposite; its circumference is nearly circular, and it stands out from the surface of the body about an inch. When the fingers are placed upon the tumour, it may be distinctly felt to pulsate, each pulsation being synchronous with one at the wrist. If the stethoscope be applied to it, a blowing sound in successive puffs is plainly audible. It is not tender to the touch. The skin over it is freely movable, and not discoloured. A bladder filled with ice is kept for two or three consecutive hours over the tumour, till the skin becomes quite numbed.

This has a very decided effect on the tumour, causing it to become hard, with the pulsation diminished.

From the history, I think you would naturally suppose the case to be one of aneurism of the gluteal artery; and for such it was taken by the surgeons who sent the patient to me from Birmingham; indeed, so certain were they, that it was considered a proper instance for ligature of the internal iliac artery. I have never seen an aneurism of the gluteal artery, and I believe spontaneous aneurism of that artery to be extremely rare.

As a young surgeon, I was early called upon to give my opinion regarding a somewhat similar tumour, situated on the back of the pelvis. My particular attention, I remember, was directed to the great exhaustion and emaciation of the patient, such as is seldom found in conjunction with ordinary aneurismal disease. Ligature of the common iliac artery was suggested, but I did not sanction its performance. The patient ultimately died; and the disease was found to be, not an ordinary aneurism, but a large, soft tumour of bone. Within the last twelve months I have been consulted concerning a pulsating tumour in the popliteal space, greatly resembling a popliteal aneurism; but, from the condition of the patient, and other symptoms, I was led to believe the disease to be a malignant tumour of bone, in which opinion I was supported by Sir B. Brodie. The patient has since died; and the opinion I offered was found to be correct.

But to return to the case under consideration. This person had been sent to me as a private patient; and, on my first visit, I found him in a very weak and emaciated condition, with various important indications sufficient to raise doubt in my mind as to the accuracy of the opinion that had been given. Finding it needful that he should remain in town for some time, I strongly advised his removal to the Hospital, where he would receive every attention, and where you, as well as I myself, might have the opportunity of studying a disease which is comparatively of rare occurrence.

After the admittance of the patient into the Hospital, I made a careful examination; and this is what struck me,—Here was a man lying in bed, with a large pulsating tumour, resembling in many ways an ordinary aneurism; pallid, thin in the limbs, hardly able to walk, and experiencing great pain in and about the region of the tumour. Now, contrast this with a case of spontaneous aneurism of the popliteal or other large artery of the extremities, but especially with one situated within one of the large cavities. The patient labouring under the disease is much annoyed at being kept to his bed; he imagines that little is the matter with him, for he may be capable of undergoing great fatigue, perchance walking great distances to seek opinions regarding his malady. In fact, it is not a feature in ordinary aneurismal disease for the patient to be out of health. I mentioned that great pain was very early complained of in the tumour, whereas it is rare for those labouring under an aneurism of a large artery to make such a complaint at so early a period.

Regarding the character of the tumour: Where I made pressure in the site of the gluteal artery, as it leaves the sciatic notch, and turns upon the outside of the pelvis, all pulsation in the tumour ceased, which fact certainly seemed very strong in favour of the supposition of a gluteal aneurism, and an indication which, taken by itself, was not unlikely greatly to bias the surgeon in his diagnosis; but I confess this had little or no weight with me, for I have met with malignant tumours of bone in various parts of the body possessing great pulsation, which could be arrested by pressure on the main artery supposed to lead to the disease.

The size of the tumour and its situation were very much against it being considered an aneurism of any one branch of the gluteal; indeed, I could not fancy such constitutional disturbance to be dependent upon such a source. It is seldom that the surgeon is consulted about an aneurism at so early a period as this case came under notice; but, if he should meet with such a disease, at what is termed its first stage, it is generally possible to empty the blood out of the tumour; but, as you saw during my visits to the patient, I frequently tried so to do, but was always unsuccessful. In the earlier stage of the disease, the crest of the ilium could be distinctly traced above the upper border of the tumour; but, after a short time, the part became entirely involved in the disease. There was one feature in which common-place anatomy greatly assisted me. I noticed that the insertions of the long extensor muscles of the back, as the disease increased in its dimensions, became elevated above the surface of the tumour; whereas, if the swelling had been an ordinary aneurism of the gluteal artery, they would doubtless have been beneath. These were, then, the principal points from which I framed my diagnosis, in the early stage of the disease; and I came then to the conclusion, that the man was not the subject of a gluteal aneurism, as was at first supposed, but that he was labouring under malignant disease of the ilium, and that the tumour was what is commonly called a pulsating tumour of bone, possessing more vascularity than was usual in such cases; and hence the well-marked pulsation.

This so-called aneurism of bone is, in my opinion, nothing more than aneurism by anastomosis in hard structure, somewhat modified in its condition and symptoms, and which might in many instances be called medullary sarcoma, or fungus hæmatodes. There is generally a very large expansion of bone, seeming as if the bone itself had been blown up, the interior of which may contain a quantity of medullary or melanotic matter, or resemble a sarcomatous mass, containing cysts with fluid blood, and possessing great vascularity. Though thoroughly satisfied in my own mind as to the nature of the disease, I was anxious, nevertheless, to avail myself of the advantage of other opinions; and, accordingly, after requesting the opinions of my colleagues, I invited surgeons of the highest eminence in London to see the case, the majority coming to the same conclusion that I have already expressed.

It was quite evident that an operation could in no way benefit the patient, and that the only treatment that could avail was rest and kind attention to his wants. He daily grew weaker, until at length, becoming delirious, ultimately died from pure exhaustion. Here let me impress upon you how important it is to avoid all surgical interference in those instances in which such a proceeding is unlikely in any way to better the patient, but which, on the other hand, is calculated to bring discredit on the surgeon, and the worst possible result to the sufferer. This disease so nearly resembles gluteal aneurism, that surgeons of experience have actually mistaken its true nature, and treated the case as if it really had been an ordinary aneurism. Among such instances I may mention two cases which occurred to Mr. Guthrie and Mr. Stanley, in which the main artery was ligatured; but subsequent results showed the error that had been committed. These gentlemen have candidly laid the whole proceedings before the profession, and therefore I have no hesitation in mentioning their names.

By studying these unfortunate cases, which are justly held out to us as landmarks, we are enabled to avoid falling into similar errors; and I often think that in our profession as much is learned from mistakes as from the performance of good surgery.

Some time before the patient's death, while paying my visit one day, I found that the left thigh-bone had spontaneously broken just below the trochanter, no cause being assigned for the accident. Now, in malignant affections, or certain forms of cancer, it is not by any means uncommon for bones to become friable, and break with the least possible exertion. This was, therefore, another very conclusive proof in support of the malignant nature of the disease.

As I have mentioned that this case was mistaken for one of aneurism of the gluteal artery, it may be advisable to say a few words regarding the treatment of that formidable disease. There are few cases on record of spontaneous aneurism of this artery, although, from its situation, it is liable to injury, which may demand surgical interference. We are all familiar with John Bell's celebrated case of wound of this vessel, in which he cut into the enormous tumour formed by the accumulation of blood under the skin, turned out the clots, and secured the bleeding vessel. But the procedure that is more in accordance with the established rules of surgery in cases of spontaneous aneurism is that recommended by Dr. Stevens, late of Santa Cruz, author of the treatise lately published on the saline practice in cholera, who succeeded in curing a large aneurism of this artery by placing a ligature on the internal iliac. Had the case we are considering been of this nature, I should have adopted the plan of treatment recommended by Dr. Stevens.

These were the principal points of interest in this remarkable case; and the *post-mortem* examination proved the correctness of the leading opinions that had been formed during life. Most of

you saw that examination; but I will briefly detail the morbid appearances which were presented. When the body was put on the table, you saw the extreme emaciation to which the patient had been reduced by the malignant tendency of the disease. On reflecting the skin from the left buttock, and dissecting back the glutous maximus, which was greatly wasted, a large tumour was found, reaching from the sciatic notch to the brim of the pelvis,—in fact, covering the whole of the haunch bone, and resembling more the substance of brain than any I remember to have seen. The tumour, I think, might with propriety be called by half-a-dozen different names, each being equally applicable, as medullary sarcoma, encephaloid disease, soft cancer, &c.

The disease had evidently commenced in the cancellated structure of the ilium, and found its way into the cavity of the pelvis, as well as outwardly. That part projecting into the pelvis had elevated the iliac vessels, and put upon the stretch the large nerves of the sacral plexus, which fully accounted for the constant pain complained of in the thigh, extending to the foot. That portion of the ilium in which the disease commenced was entirely destroyed, the free margin of the bone being left rugged and bare.

The gluteal artery, much enlarged, could be traced, shortly after leaving the sciatic notch, winding over the lower part of the tumour, and distributing large branches, which ramified freely over the surface, and fully explained the reason of the well-marked pulsation during life. The large sacro-sciatic nerve was pressed upon by the lower part of the tumour, which doubtless caused the excessive and frequent pain to the patient. The disease had extended to the left side of the two lower lumbar vertebræ, sacrum, crest of the ilium, and upper part of the femur. The remaining bones of the body were not found similarly affected, but the kidneys and lungs presented well-marked specimens of cancerous deposits.

I have been particular in directing your attention to this case, both because it was fraught with interest, and that it created as much thought and speculation in my mind as any I can at the present time remember.—*Med. Times and Gazette, Dec.* 9, 1854, *p.* 585.

60.—ON TUMOURS OF THE UPPER JAW.

By Henry Hancock, Esq., Lecturer on Surgery at the Charing Cross Hospital.

[It is a common opinion that these tumours originate within the antrum of Highmore. Mr. Hancock does not agree with this opinion. Mr. Ormerod, of St. Bartholomew's, has taken pains to classify these tumours, which he does as follows:]

1. Epulis.
2. Cystic tumours, consisting of the walls of the bone expanded on

or into a sac, with more or less of solid growth; the walls in some parts being quite membranous and transparent, and the sac filled with glairy fluid, or a firm substance contained in cells, or accompanied with a granular fatty substance, partitioned by fibro-cellular substance; also consisting of a single bony cyst lined by a membrane, with a second canine tooth adherent to it, and having its cavity filled by a glairy fluid.

3. Cartilaginous and osseous tumours from the upper jaw-bones, as round tumours growing on their outer surface here and there, but chiefly as a mass growing form their inner surface, so as to fill up the maxillary sinuses, the septum nasi and spongy bones being also affected. The thickening is dense, hard, and ivory-like.

4. Fibrous tumours from the upper and lower jaw-bones, consisting of white or pale yellow, firm, and nearly homogeneous, with or without specks of bone, or of dense more or less fibrous, or even obscurely fibrous, substance, containing at times one or more small cavities filled with pus, with a glairy fluid, or with blood; growing on a healthy bone and periosteum, or from the alveolar and outer surface of the bone, or originating in the cancelli, and being accompanied with a perfectly healthy condition, with absorption or consolidation of the surrounding bone.

5. Medullary tumours from the upper or lower jaw, consisting of round, lobed, or nodulated masses, with a smooth membranous covering, or with a rough fungous or ulcerated shreddy surface; invested by a thick capsule by a dense periosteum, or by partial thin laminæ of bone; composed of round lobes, connected together by cellular tissue, or formed of an almost homogeneous mass; made up of a structure of a soft medullary, brain-like, spleen-like, or firm fatty nature, with cells and bony spiculæ, sometimes commencing in the neighbouring glands, and extending to the jaws secondarily.

In addition to these, Mr. Stanley enumerates fatty and erectile tumours of the jaw; whilst Mr. Paget adds what he terms myeloid tumours of the part; but whilst the examples he quotes resemble so much, on the one hand, the fibrous, on the other medullary tumours; their true character, whether innocent or malignant, is so very doubtful, that I should hesitate in admitting them as a distinct class.

I have never met with a fatty growth so invading the upper jaw as to require the removal of any portion of that bone, and therefore I do not presume to offer any opinion upon that form of disease.

Mr. Stanley states that the erectile tumour is a very rare form of disease; but that it occurred to M. Gensoul in one of his successful cases. The tumour was soft and vascular, and quicksilver, impelled into the morbid structure, readily pervaded it throughout. I have not seen a similar case, nor have I been able to meet with any recorded elsewhere. It is occasionally very difficult to form a correct opinion of the exact point of origin of a fibrous tumour of the upper jaw. It will sometimes commence by a contracted pedicle, enlarge, and spread out, and not only fill the antrum, but extend into the nares; by its

peculiar and irregular appearance giving rise to the supposition of its being a polypus of the nostril, and leading to abortive attempts for its removal by the polypus forceps." Mr. Stanley although not mentioning the disease, has the following observations bearing upon this point; —"A tumour gradually increasing within the antrum may occasion yielding of the walls equally in all directions; but in some cases, disease extends chiefly in one direction, and causes difficulty of diagnosis. In a case I saw, a morbid growth originating in the antrum had extended only in the direction of the nostril, and portions had been extracted by polypus forceps."

[Osseous tumours are the most frequently met with amongst the non-malignant growths of the upper jaw. Mr. Hancock has two specimens of this disease.]

The first case is that of a young man, aged 22, a farmer, residing in Lincolnshire, who was sent up to me on the 1st October, 1848, by Mr. Young, of Gosberton, whose patient he was. This case was a striking example of what has been affirmed elsewhere, "that osseous growths are in some instances combined with hypertrophy of the surrounding bones, producing general enlargement." The whole of the bones on the right side of the head and face were very much enlarged and thickened, the bone at the fronto malar suture presenting a surface of more than a square inch, and this, I believe, is the usual origin of the tumours in question. I believe that they should not be regarded as originating within the antrum, or from causes connected with the teeth, but from excessive development or hypertrophy of the bone itself, which, in its growth, extends in various directions, invading the antrum of Highmore, and in some instances entirely obliterating that cavity. Mr. Paget, in his Lectures on Surgical Pathology, in allusion to these tumours, says, "More commonly, it is almost limited to the antrum"; but he singularly disproves his assertion, and confirms my views, by his illustrative example. He says, "In this case, it may exist with little deformity. In the museum of St. Bartholomew's Hospital is a specimen, in which both the antra appear nearly filled by the thickening and in-growing of their walls; only small cavities remain at their centres. The new bone is hard, heavy, and nearly solid, yet it is porous or finely cancellous, and is neither so compact nor so smooth on its cut surface as that of 'ivory exostosis.' The same disease is manifest in a less degree upon the outer surfaces of the maxillary bones, and on the septum and side walls of the nose."

I have operated upon three cases of this form of disease. One of these I have just related, and in the other two the antrum remained; in one of these there was general hypertrophy of the bone, causing great disfigurement, and necessitating its entire removal. The swelling did not arise from, and was not confined within, the cavity, but was, in truth, a portion of the general thickening and enlargement of

the bone. I have a preparation, which was taken from a child of six years of age. sent to me by Mr. Calthrop, of Gosberton, Lincolnshire, upon whom I operated in April, 1852, and is a well-marked example of the fallacy of Mr. Stanley's position, that "morbid growths mostly arise from either of the lateral parts, not from the front of the jaw—a fact which might be explained by the consideration that irritation more frequently originates in a molar than in an incisor tooth," as does the following case:—

A child was admitted, under my care, into the Charing-cross Hospital, in Aug. 1852, with tumour of the left upper maxilla. It appears that in Dec. 1851, she fell and bruised her face. Soon afterwards a tumour was observed on the left side, just below the orbit. This was unattended with pain, but it gradually increased in size until, at the time of admission, it was about the size of a walnut. Upon careful examination, I found the hard palate and gums perfectly healthy; the tumour was smooth and solid. Upon carrying my finger behind the soft palate, I could not detect anything wrong in that situation; but as I otherwise could not detect the extent of the mischief, or what was the extent of operation required, I introduced a small exploring trocar into the tumour, and felt it enter a solid mass, which prevented my giving the instrument any lateral motion. I next perforated the upper jaw above the alveolar process corresponding to the molar tooth, and found the instrument enter a cavity, in which I could freely move its point, and from which I decided the case was hypertrophy of the anterior portion of the maxillary bone, and I acted accordingly, confining the operation to the simple removal of the part affected, without interfering either with the floor of the orbit or the roof of the mouth.

The last form of abnormal growth is the encephaloid or medullary sarcoma; and it is here that a correct idea of the origin of these growths becomes of so much importance. I have throughout this paper been at great pains to disprove the opinion so universally maintained, and so decidedly advocated by Mr. Stanley, that morbid growths commonly originated within the antrum, not only because I felt, from what I had observed, that such opinion was wrong, or rather open to very many exceptions, but because the statement, backed by such an authority, gives rise to the supposition that medullary sarcoma usually commences within the antrum; and that, if attacked sufficiently early, it may be eradicated by operation. At the risk of appearing tedious, I feel that the importance of the subject fully justifies my again quoting Mr. Stanley. He says, "But even with the help of the most careful examination, whenever the disease fills the antrum and nostril, it will be uncertain whether or not it extends posteriorly beyond the front surface of the pterygoid processes of the sphenoid bone. Whilst Mr. Liston has observed, 'If anything is to be done, it ought to be undertaken with a thorough determination to go beyond the limits of the morbid growth, to remove the cavity which holds it,

and thus get quit, if possible, of all the tissues implicated, or which may have become disposed to assume a similar action.' I know from experience that this step, if adopted in time, may prove successful." It may appear presumption in me to differ from such authorities as these, and men of such experience, but my own experience leads me to an opposite conclusion.

I entirely differ from the opinion that medullary sarcoma commences in the antrum of Highmore, and extends backwards towards the pterygoid processes; but, on the contrary, from what I have observed, I firmly believe that the disease commences in the cancellated structure of the body of the sphenoid bone and bones at the base of the cranium; and that, however early we may perform the operation, we never succeed in eradicating the mischief, which is sure to return at a longer or shorter period, according to circumstances.

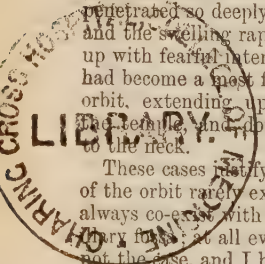
I have performed and assisted at many operations for the removal of the upper maxilla for this disease, and where the mischief, as far as external examination and the introduction of the finger behind the soft palate went, appeared to hold out every reasonable prospect of its complete removal, but in no one instance was this satisfactorily accomplished, and in no one instance did the disease fail to reappear; whilst post-mortem examination demonstrated its growth from the body of the sphenoid bone, the basilar process of the occipital bone, &c. This disease will sometimes extend into the orbit, in some instances causing the eyeball to protrude, in others presenting us a firm tumour on one side of the eyeball. But it should be borne in mind that, notwithstanding the absence of external swelling of the cheek, these growths rarely, if ever, originate in the orbit, or are confined to that cavity, but that the swelling in the orbit is almost invariably an extension of the disease in that direction, its origin being in the cancellated structure of the bones at the base of the cranium, and that it mostly, at the same time, invades the spheno-maxillary fossa and antrum of Highmore, so that it can never be entirely removed by dissection from the orbit.

I have a lady at present under my care, who consulted me for protrusion of the left eyeball, with apparent commencing chemosis of the conjunctiva and indistinctness of vision. The protrusion is not very great; but pressing the base of the eyelids near the margins of the orbit gives the sensation of an elastic body between the orbit and the eyeball. There is no swelling of the cheek; but upon examining the mouth I find the gum is beginning to separate from the molar teeth, and the dental margin to become everted, whilst separation is also commencing between the hard palate and its covering, the bone appearing softer than natural when pressed upon. Carrying my finger behind the soft palate into the spheno-maxillary fossa, I can distinctly feel an irregular growth in that situation.

This case shows the impropriety of operating upon orbital growths of this description, the tumour in that situation being commonly but

a local manifestation of disease extending into the upper jaw and elsewhere, where the knife cannot penetrate. In some instances the tumour will be more isolated or partial, and equally, if not more malignant, if we may judge from the rapidity of its growth. It will not present the same elasticity to the touch, feeling more like an enchondroma, and when cut into, it has very much the appearance of a fibrous growth.

A woman brought her child, aged two years, to me at the Ophthalmic Hospital with a conical tumour of the orbit, at the inner and upper part; it was very firm to the touch, and the mother said it caused the child great uneasiness. She had perceived the swelling about a month before. I entertained some doubts as to the nature of the swelling, and expressed them to the mother, at the same time telling her that I could not recommend any operation. At this time the disease was apparently confined to the orbit, as by careful examination I could not detect any manifestations elsewhere. She pressed me to do something, and therefore I endeavoured to remove it, and found it to consist of dense fibrous structure, invested by yellow, firm substance; it penetrated so deeply, however, that I could not get the whole away, and the swelling rapidly reappeared, the disease seeming to be lighted up with fearful intensity. The child lived two months afterwards, but had become a most frightful object. The tumour protruded from the orbit, extending upwards towards the forehead outwards, invading the temple, and downwards through the upper maxilla and mouth to the neck.



These cases justify the opinion that fungoid or medullary growths of the orbit rarely exist as independent diseases, but that they almost always co-exist with the same disease in the antrum and spheno-maxillary fossa; at all events I do not recollect an instance where this was not the case, and I have seen many of them.

Having endeavoured to point out the inadvisability of performing operations in cases of medullary sarcoma of the upper jaw, I will now advert to some of the most prominent means of diagnosis between this disease and other abnormal growths of that part. The diagnosis between fibrous tumours and medullary sarcoma is very obscure: some idea may be obtained from puncturing the tumour, and from the degree of resistance to the lateral motions of the instrument presented in the former disease, as well as from the absence of spiculæ of bone discernible in some forms of the latter; but the principal diagnostic signs which we possess are, the condition of the teeth, gums, and bones of the part. It is true that the bones, in certain conditions of health—as in the case to which I have alluded—may, from pressure, become carious, or even destroyed by necrosis; but they are never converted into that soft degeneration so characteristic of medullary sarcoma, or the malignant disease. Pressure on the hard palate is met with firm, hard resistance; whereas in medullary sarcoma—in all cases where the tumour projects from the cheek, and in very many of those wherein

the disease manifests itself in the orbit—the palate upon the affected side is elastic to pressure, usually more congested than natural, whilst the gums are congested and thickened, the dental margin is thickened and everted from the teeth, which become irregular, loosened, and projecting. When seen for the first time, this condition of the gums and teeth may be mistaken for abscess or disease connected with the teeth, with which, however, it has nothing to do, and from which it may be distinguished by the comparative slight pain which accompanies it, by its gradual progress, by the absence of inflammatory swelling of the cheek, and by the character of the enlargement of the gums.

In this affection the gums are spongy and elastic, and, when pressed upon, are found, in a great degree, to have lost the support of the alveolar process, which becomes softened by the disease; whereas in inflammation and abscess resulting from disease of the teeth, or alveolar sockets, of a simple character, the pain is very great, the swelling of the cheek is evidently of an inflammatory character, whilst the enlargement of the gums is smooth and firm, and the firm resistance of bone beneath may almost always be detected. In medullary sarcoma this state of the gums should be regarded as the result, not the cause, of the disease; and for the reasons which I have already given in the early part of the paper, the teeth, though loose and comparatively useless to the patient, had better not be meddled with.

Bony tumours, though more easily distinguished from medullary sarcoma than fibrous growths, may, upon superficial examination, create doubts in the minds even of well-informed surgeons. As I was going down into the operating-theatre, to remove the upper jaw from a child aged six years, a surgeon of some standing, who had examined the child in the ward, said, "Surely, Mr. Hancock, you will not meddle with that; it is malignant." He was misled by a certain degree of elasticity presented at one point of the tumour, and which I have observed in all the cases which have fallen under my observation. They have not been of that form denominated ivory exostosis, but have all presented a certain degree of yielding to the touch; but they have this marked distinction from the medullary sarcoma: in this latter disease the surface of the tumour is uneven and elastic, but the elasticity is superficial; whereas in osseous tumours the elasticity is deeper seated, and the yielding or elastic substance is evidently covered by a thin shell, which recedes when pressed upon, but appears to recover itself when the finger is removed. We have not the crepitation in these cases as in osseous cysts, but the elasticity is covered by a thin, smooth investment, and is not so immediately upon the surface as in osteo or medullary sarcoma. Puncturing these tumours with an exploring needle, or, what I prefer, a small trocar, will also assist us in our diagnosis. In medullary sarcoma, the instrument appears to enter a soft mass, and with but little force a considerable degree of lateral motion may be given to the instrument; but not so in osseous tumours. Here, though it may be introduced without difficulty, still it will ap-

pear to be clung to by the structure into which it penetrates, and lateral motion cannot be obtained except with that degree of violence which we should be unwilling to employ; and, again, as in fibrous growths, the condition of the gums and teeth will pretty surely mark the non-existence of malignant disease. The teeth will, in many instances, be perfectly sound; and though, in others, the patient may have lost some of those organs, the gums will have healed, and will not show any of that spongy appearance, or tendency to eversion, so characteristic of the malignant disease. I never saw a tumour of the upper jaw of a malignant character unaccompanied by mischief about the palate and gums; but an osseous tumour may have attained great size, and cause considerable deformity, without projecting into the mouth, or complicating the palate or gums at all; and this is an additional reason for employing an exploring needle or trocar, this instrument, in some cases, affording us the only means of ascertaining the extent of operation to be performed—whether the whole, or a portion only, of the upper maxilla is to be removed. Mr. Fergusson has laid down that rapidity of growth is a diagnostic sign of malignant tumour; this, however, is not entirely to be depended upon, as the growth of bony tumours will sometimes be extremely rapid, as in one case wherein the tumour attained a great size in less than five months.—*Lancet, Jan. 6, and* 13, 1855, *pp.* 5, 30.

61.—TUMOURS OF THE JAWS.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[In a clinical lecture on this subject, Mr. Syme makes the following important general remarks, which will apply to tumours affecting both upper and lower jaw.]

There are some anatomical differences between them, which lead to pathological differences. In the upper jaw, the hollow of the maxillary antrum exposes to a condition which cannot exist in the lower jaw—viz., inflammation and suppuration of the cavity. The antrum also serves as a receptacle for polypous growths, which, however, are mere intruders from the nasal cavity, and not diseases of the jaw itself. In both jaws the bone is apt to die from the effects of inflammation. In the upper no reproduction of bone takes place after removal of the exfoliation; in the lower the bone is denser, and the periosteum more capable of reproduction, and there is no instance in the body of a more complete restoration of bone. I have repeatedly seen almost the whole lower jaw, including the condyle, thrown off as an exfoliation, yet complete reproduction has taken place. These are all the differences which it is necessary for me to notice now. There has been a notion prevalent, that tumours of the upper jaw originate in the antrum. This is erroneous; their origin is in all cases similar to tumours of the lower jaw.

Tumours of the jaws are chiefly of five different kinds—three solid and two fluid. Of the solid, you have one growing from the surface, and two in the interior of the bone. The one on the surface is denominated "epulis," or "epulotic tumour;" it appears on the gum, and increases in size without limit to its growth, and at last becomes inconvenient from its bulk; it is also liable to bleed, and may become as serious, in this respect, as if it were malignant. Epulis grows from the surface of the bone, and requires for its remedy the removal of the part to which it is attached. The knife never cures the disease completely, and it is always requisite to take away more or less of the alveolar process, in order to do which you must extract one or more teeth.

The other two forms of solid tumour originate in the osseous substance, and are divided into simple or local, and malignant, or those where there is some constitutional derangement connected with the local condition, which produces a tendency to a return of the disease after its removal. The first kind are generally of firm consistence, and slow growth, and accompanied by a healthly state of the constitution; the second are of soft consistence, and rapid growth, and the patient's constitution commonly unhealthy. But these kinds pass, by various degrees, insensibly into each other. The slower the growth, the firmer the consistence, and the more healthy the constitution, the greater will be the probability that the disease is entirely local, and that the removal of the tumour will be followed by a permanent cure. But tumours of the most rapid growth and softest consistence have sometimes been taken away, without any return of the disease; and hence the general rule is, that if such tumours are distinctly limited to the jaw, they should be removed. You must bear in mind, however, that a malignant growth is apt to extend its roots beyong the bone from which it originated, in this respect remarkably differing from the simple, local, or fibro-cartilaginous growths, to which your attention was lately directed. The other day, a lady, said to be labouring under polypus of the nose, consulted me. I found, however, that the soft mass which showed itself in the nostril was really a tumour proceeding from the superior maxillary bone; and further, a swelling at the inner canthus showed that the disease was not limited to that bone, but extended up to the ethmoid, and probably to the base of the skull. I therefore advised her against any interference; but had the disease appeared limited to the superior maxilla, I should have recommended excision of the bone. I could mention several cases, where the removal of soft tumours of the jaws of rapid growth, has been followed by permanent recovery.

The fluid tumours of the jaws are of two sorts, being collections either of serous or purulent fluid; the serous or cystic are not unfrequently mistaken for solid tumours; they grow in the substance of the bone, and are generally at some parts of their extent as hard and as unyielding as bone, so that if their examination be limited to these

parts, the tumours will be supposed to be solid. Even so accomplished a surgeon as M. Gensoul, of Lyons, had once made his incisions and exposed a tumour of the jaw, when all at once the knife entered a cavity, from which serous fluid escaped. I have myself met such cases, and seen them in the practice of others. It is therefore necessary to examine a tumour of the jaw over its whole surface; then, if it be cystic, you will be almost sure to find some yielding point where the bone gives way like pasteboard, and is perhaps felt to crackle under the finger.

These cysts frequently exist independently of any other morbid condition, but sometimes, and especially in the upper jaw, they are connected with displaced teeth, one of the permanent set having taken a wrong direction, so that, when the cyst is opened, the tooth is found lying in its interior: I have, in this theatre, more than once found such a state of things present. Whether this be the case or not the treatment is the same—viz., to open the cavity freely and stuff it with lint. The prognosis is, however, more favourable if a tooth be found in the cyst; it is then almost certain that the cavity will contract and heal. This may occur in other cases, but it has also been noticed that cysts, after being opened, are not unfrequently followed by the formation of a solid tumour. I could mention to you cases where I have opened cysts containing nothing but serum, in the place of which solid tumours have afterwards been formed. It is also possible that the cysts may be complex, and so extensive as to require removal. A patient left the hospital only a few weeks since who applied to me five years ago on account of a large cystic tumour of the lower jaw, which I opened, and gave exit to a large quantity of albuminous fluid, and stuffed the cavity with lint. The swelling afterwards contracted to a considerable extent, but never entirely disappeared, and she returned two years later with the tumour as large as it was when I first saw her, and affecting the jaw more extensively. I again opened and stuffed it, and the tumour again contracted to a certain extent. It, however, again increased, and I repeated the process for the third time, but with the same result, the patient returning last autumn with the tumour larger than ever, and extending now from the symphysis to the coronoid process. I removed half the jaw by disarticulation, and on examination we found that the cyst, instead of being simple, was complex, consisting of four chief cavities, the walls of which were studded with smaller cysts of various sizes. It was clear that in this case nothing short of excision of the portion of bone affected could have produced a cure.

Tumours depending on purulent collections are not at all uncommon. They are almost invariably in connection with the stump of a tooth. A portion of a stump may remain after the extraction of a tooth and be forgotten, and the tumour may be supposed by the surgeon to be solid. Generally, however, the parietes, like those of serous collections, will be found to yield at one place, and you find an additional guide

to your diagnosis in the presence of the diseased tooth. I lately saw a tumour of the lower jaw, which was removed under the supposition that it was solid throughout; but on being sawn through, it proved to be an abscess, the osseous wall of which had undergone immense thickening, except at one small point in immediate connexion with a diseased tooth, which had never caused any pain. In that case, it may be doubted whether it would have been possible to afford relief otherwise, even if the real nature of the disease could have been made out, from the difficulty of affording a free outlet for the discharge.—*Lancet, March* 10, 1855, *p.* 253.

ORGANS OF CIRCULATION.

62.—ON POPLITEAL ANEURISM.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[In a clinical lecture on this subject, in which will be found some valuable hints as to the mode of tying the artery, and especially with respect to meddling with the vein, Mr. Syme speaks as follows:]

We are called upon to adopt some means of treatment, and you are aware that there are two methods for us to choose between—namely, ligature of the vessel, and the application of pressure over it. It may be said, in favour of the former, that it causes no pain whatever; since, if chloroform is used, the patient does not feel the operation, and he suffers nothing after it, his only duty being to keep quiet for a fortnight or three weeks. The advantages alleged in favour of compression are, that it is safer, ligature being attended with risk of hemorrhage or of mortification of the limb, while compression is devoid of these dangers, and therefore preferable. On the other hand, if the operation of ligature be properly executed, it is almost absolutely free from danger; pressure, moreover, does not always succeed, even in the most experienced hands, and generally requires to be applied for weeks or months before any appreciable effect is produced by it, while the patient requires to be kept almost under military discipline, in the endurance of very considerable pain, as you will understand if you will apply a tourniquet on your own persons. In an institution like this, the length of time occupied is a serious consideration; for instance, a patient lately in the hospital, whose case has been published, was for seven months under treatment by compression, and at last ligature of the artery was resorted to. Here was a bed occupied for this protracted period which might have been the means of relief to many cases, with proportionate instruction to those who study here. But it may be said, in addition, that pressure is not quite so safe as it is generally stated to be; it may lead to mortification, and other disturbances, which have been described in some cases that have been published, and which, I have reason to believe, would be found more fre-

quent if all the cases in which compression has been tried, were placed before the public. After a careful consideration of the subject, I have come to the conclusion that, if the operation of ligature be performed properly, with due attention to the niceties that render it safe, it is preferable; but if circumstances prevent this, compression should, by all means, be had recourse to. I can understand many such circumstances; for instance, a surgeon in the public service, or some remote district of the country, who has little opportunity of practising operative surgery, may feel that compression would be the safer treatment. In short, the question seems to me very similar to that of whether a catheter should be passed, or the bladder punctured, for retention of urine; if the urine can be drawn off by catheter, by all means let this be done, but if the circumstances of the case should be such that not one surgeon in a hundred could do so, it would be incumbent on the ninety-nine to obviate the risk of rupture by puncture of the bladder; and, hence, whilst I regard the introduction of the catheter as the preferable means of relieving retention of urine, I have never undervalued puncture of the bladder as an operation in surgery. I say this the more because I read, a few days ago, in a London periodical, the statement that I "have endeavoured to cast a certain amount of discredit on those who have ably devoted their time and energy to the trial of a more simple and less hazardous cure of this formidable disease."

This is a very serious accusation against any man, and particularly so when coming from one teacher of surgery against another. It is indeed quite true that I have given no countenance to all the claptrap performances of what has in recent times been denominated "conservative surgery," but it is not true that I have in any way opposed the efforts of those who have striven to improve the practice of surgery. The same lecturer says that the treatment of aneurism by compression was "in vogue," as he expresses it, "two hundred years ago," but was "abandoned after the trial of Hunter's operation in 1780. This is entirely incorrect, and for very obvious reasons, since compression as now practised was only thought of by following out the principle of the treatment of Hunter and Desault, (for Desault's name should always be mentioned along with Hunter's in speaking of this subject.) Hunter's operation contrasted strongly in its results with the old method, which was, to cut into the aneurism and tie the vessel both immediately above and below it, a proceeding which was so rarely successful, that Dr. Wilmer, of Coventry, (1779,) says that he knew of no instance of its being so in this country. When, therefore, it was learned that the operation of Hunter was commonly followed by success, a very great sensation was produced in the medical world; but unhappily, it soon appeared that the operation was not unaccompanied with risk of hemorrhage, and you will find that various expedients were in consequence introduced with the hope of diminishing this danger. Instead of a simple piece of silk or fine thread, ligatures of

reserve, as they were called, were also passed, to be used if necessary; thick cords or tapes were employed, a pad of lint was interposed between the vessel and the ligature, the artery was exposed and forceps (*presse artères*) were applied for a time instead of a permanent ligature, and with similar views the artery was compressed at the seat of Hunter's operation, without exposure of the vessel; and if you will look into M. Boyer's Surgery, you will find two cases recorded in which this treatment had been used successfully, and in reading the accounts it will seem to you as if you were perusing recent cases instead of those half a century old. Now I beg you to notice what took place subsequently. The cases I speak of were in 1804 and 1806; in 1807 a great step was made by the publication of a work by Dr. Jones, a graduate of Edinburgh, on the process by which hemorrhage is arrested. He clearly made out the following important points: that the safety of the operation of ligature is the greatest when the artery is exposed to the smallest possible extent; also, that the risk is less the less the surrounding parts are disturbed and torn, and the more exclusively they are divided by a sharp cutting edge: further, that the ligature, which is a foreign body, should be as small as is consistent with strength, and tightly tied. It was found that if these points were attended to, the ligature of arteries was accompanied with much less risk than had previously been the case. Following out these principles, I have tied the femoral artery twenty-three times, without any bad effect. I do not by any means wish to say that every artery I happen to tie will be tied successfully, but I do say that this fact is sufficient to warrant me in maintaining that the operation, when properly performed, is not in itself a dangerous one.

Now let me explain distinctly what I mean by the operation being properly performed: I do not wish to imply something done with extraordinary dexterity, or what only a few persons can do, but I would insist upon the necessity of bestowing sufficient care upon the procedure. Take, if you please, half an hour, or a whole forenoon about it; but do not pass the needle till the artery has been sufficiently separated from the contiguous structures, to enable you to avoid all risk of injury to the vein, which is the great source of danger to be dreaded.

[In one patient Mr. Syme tied the artery on January 9th, with his usual precautions with respect to the vein. On January 18th, nine days afterwards, Mr. Syme says,]

The man who was operated upon for popliteal aneurism nine days since is going on perfectly well; there is nothing he can complain of, except confinement to bed, which will probably not last many days longer. When you see a patient labouring under a formidable disease cured by an operation which gives him, through chloroform, no pain at the time, with the expenditure of only a few drops of blood, and little or no subsequent suffering, you naturally regard the treatment as very

near perfection. Why, then, you will ask, is it not universally adopted? On account of danger to which it is said to be liable from two sources—viz., mortification and hemorrhage. I wish to consider at present how far these two sources of danger are unavoidable, and upon what they depend. And first of mortification. This is said to be due to the limb being weakened in its vitality when the artery is tied, in consequence of the blood not flowing sufficiently freely through the anastomosing vessels, and this, it is alleged, operates more especially in small aneurisms, where there has not occurred sufficient dilatation of the anastomosing vessels to form an efficient subsidiary channel. Mortification is said to happen in two ways: in one, the weakened limb is for some time after the operation cold and impaired in sensation; then a period of reaction occurs, accompanied with pain, swelling, heat, and redness; and if this reaction be excessive, it may prove fatal to the limb by the inflammation. In the other mode of occurrence of mortification, reaction does not take place, but the limb remains cold, and its colour changes to blue, its moisture exudes, and it finally becomes dry and black, like a mummy, and the death of the patient, or amputation of the limb, is the inevitable consequence. These are very frightful statements, and I confess that in the first case on which I operated I felt considerable alarm. The aneurism was of small size, and the patient (a gentleman of no great firmness of constitution originally) had been much reduced by the treatment to which he had been subjected by the late Professors Thomson and Turner—viz., confinement to bed on very low diet, with the object of promoting spontaneous cure, but with no beneficial result. To my astonishment, however, I found after the operation that there was scarcely any appreciable difference of temperature between the two limbs; that this soon passed off, and that no inconvenience of any kind was experienced. This is now, I believe, the twenty-fourth case in which I have tied the femoral artery, and I never met with any other result. There has never been any difference of temperature, except a slight coolness, which required careful examination with the hand or the thermometer to detect. There has never been any threatening of reaction, never even a partial slough, yet my patients have been old and young, (I once operated on a boy, nine years old,) and the aneurisms both small and large, as well as of recent and of long duration. I have, however, seen mortification occur in the practice of others in both the ways I have mentioned. The case with reaction was one on which Mr. Liston operated; the patient had suffered pain previously, and had been in the habit of relieving it by the use of warm baths, of which he kept an establishment. The pain continued after the operation, and the hot bathing was liberally used the same evening and next day. Swelling and reaction came on, terminating in mortification, and I assisted Mr. Liston in amputating the thigh at the middle. In other cases I have seen the limb become cool after the operation, and in the evening cold as a dead body, then blue, and finally mummified. Now there

must be reasons for results such as these, and I confess that this was to me at one time a subject of very anxious inquiry; but all my own investigation would perhaps have been vain, had it not been for the observations of Mr. Guthrie, which are important on many points, but especially on the subject of injuries of arteries. He observed that the artery alone might be obstructed without death of the limb, but that this was the invariable result if both artery and vein were obstructed together.

[Mr. Syme has long been of opinion that where the results of this operation vary so much from his own experience, it is owing to some intermeddling with the vein. He says:]

You are aware that the femoral vein is intimately connected with the artery, much more so than in the case of some other vessels that may require ligature—*e. g.*, the iliac artery is but loosely connected with the vein, and great freedom, or even great roughness, may be used with it without bad result. It is not so, however, with the femoral; if you do not detach the artery from the vein with the greatest nicety, you will very likely either pierce or bruise the latter. But if you do the operation in the way that I shall presently describe, you may fairly expect that, instead of mortification, either with or without reaction, the only change that will occur in the limb will be a slight diminution of temperature, productive of no inconvenience, and which will disappear in a few days.

Now, we come to the other source of danger—viz., hemorrhage; and in order to consider this, I must call your attention to the process of separation of the ligature. The first effect of the ligature is to cut through the internal and middle coats, and to draw the external coat into a small compass, not wider than a probe. The next result is the coagulation of the blood lying in the artery near the ligature, forming a conical coagulum on each side, tapering away like a worm as far as the nearest large branch. The next change is an effusion of lymph on each side of the ligature, sealing up the ends of the vessel; then a process of ulceration takes place for the separation of the foreign body viz., the ligature, with the portion of external coat included in it. If you examine the ligature when it comes away a few days hence, you will find that it includes a substance, which is the piece of external coat that has died, in consequence of the constriction it has suffered. If the sloughing or ulceration go beyond due bounds, the lymph will be no longer able to prevent the clot from being forced out by the heart's action. The clot could do little good of itself, but it diminishes the force of the blood upon the barrier of lymph, and the longer the coagulum, the more effectual will it be in this way; hence the further the ligature is removed from a large branch, the greater will be the patient's safety. Now, the femoral artery varies in its point of origin from the common femoral, and it is very important that the ligature should be applied considerably below its lowest origin; hence one great

element for safety is to tie low enough, and I do not doubt that some cases of hemorrhage have occurred in consequence of the ligature being applied too high up. I have seen many persons perform this operation at a higher point than I am in the custom of doing, which is where the artery has become overlapped by the sartorius muscle. The operation is rendered a little more difficult by the overlapping of the muscle, but this is altogether a secondary consideration. There is, however, a limit to the distance downwards: if you go beyond the sartorius, as Hunter did, you will be too near the aneurism. Whatever tends to denude the artery from its connexions will increase the extent of sloughing of the arterial coats; and whatever bruises or lacerates the surrounding textures, and so interferes with union by the first intention, will have the effect of exposing the vessel, subsequently to the operation, during the process of suppuration that will ensue.

In performing the operation, I recommend you to use nothing but an ordinary knife, a pair of dissecting forceps, a needle for passing the ligature, and some hooks for holding the parts aside. Do not allow your assistant to draw the skin towards him while you make your incision, but, in order to keep the integument from slipping, place your hand across the thigh, and cut from it along the course of the vessel. After opening the fascia, you may perhaps be immediately upon the artery; this you may ascertain by feeling for its pulsation with your finger, which may be used for this purpose, but never for separating the textures. I think it better to divide the fascia over the sartorius, which is recognised by the direction of its fibres, when a little dissection brings you into the muscular interspace. I beg now to show you a thing, which you may think of small consequence, but which I have found very useful—viz., this form of hook for keeping the parts aside. I met with it accidentally amongst a set of old instruments, but it has proved of great service, as it not only holds the muscles to one side, but also presses them down; and in this respect the difficulty of the operation is greatly diminished by chloroform, which prevents the tension of the muscles which occurs when the patient is conscious. You will find in this, as in many surgical operations, that the result often depends upon some apparently trivial matter, on which, however, turns the whole question of a successful or fatal issue.—*Lancet, Feb.* 10, 1855, *p.* 145.

63.—ON THE TREATMENT OF ANEURISM BY COMPRESSION.

By Wm. Fergusson, Esq., F.R.S., &c.

[On the practice of compressing the main artery leading to the aneurismal tumour, Mr. Fergusson makes the following remarks:]

No doubt this plan is far less brilliant in its details than a daring cutting operation; and more credit is due to those who were the

means of its revival, when it is remembered, that it was resorted to at a time when a successful operation was in continual practice with the most satisfactory results.

The advantage of compression in the treatment of external aneurism, when practicable, over that by ligature, is at once evident. By inflicting one kind of injury, we are often enabled to get rid of a still greater mischief, and this is considered good surgery; but if, by a process less dangerous and hazardous, and without performing a cutting operation, the same disease or injury can be cured, surely it is a better style of surgery, and one that approaches nearer to the perfection of our art.

The history of the case is short; it is briefly this:—A. B., aged 22, married. Has always enjoyed pretty good health. The first symptom of her disease occurred twelve months ago. This was pain, confined to the popliteal space of the left leg; she could assign no reason for its coming on, as she never had any accident to that leg. The pain, which has lasted to the present time, occurs in paroxysms of variable length, sometimes lasting for a whole day, and at other times not an hour, and accompanied with intervals of perfect ease. The pain is always aggravated by exercise, or by merely extending the leg. It has never been bad enough to prevent her following her ordinary occupation. Soon after its first occurrence, she noticed a hard, throbbing tumour, the size of a hazel-nut, in the centre of the ham; it was not tender to the touch, although it seemed to be the seat of the pain. It has remained almost unchanged since its first appearance; the only alteration that has taken place is a trifling increase in its size. It is quite firm and pulsating. Pulsation is completely arrested, if the superficial femoral be compressed. A bruit is heard on applying a stethoscope to the tumour.

There are one or two very singular features in this case. The first is, the occurrence of a popliteal aneurism in so young a person. You may never again see a similar case. I never met with one before in so young a subject, and I imagine hardly a similar instance is to be found in the records of English surgery. The chief point, however, to which I wish to direct your attention is, the plan of treatment that was pursued. It is one that was in vogue with surgeons two hundred years ago, and various accounts of it are to be found in the works of older authors. After the successful trial of Hunter's operation it was given up; but within the last few years it has been revived by some of the most eminent surgeons, especially those of Dublin, who have taken precedence in this interesting subject. A full account of the practice will be found in some of the established modern works, and a slight sketch in my own book on 'Practical Surgery.' Since the revival of this method, there has been sufficient success to strike most surgeons with astonishment; for out of a long list of cases treated by compression, success has been great beyond anticipation, and much greater than when the disease had been subjected to the treatment by ligature.

This method of treatment is now very generally sanctioned by the profession, and I am not aware of any distinguished surgeon, save one, who strongly objects to at least a trial of this plan. I allude to Mr. Syme, who has been remarkably successful in a great number of cases in which he used the ligature; for he states that in tying the superficial femoral for the cure of popliteal aneurism in upwards of twenty cases, he has not met with a fatal result. But I have seen the artery tied equally well by other surgeons of eminence, in well-selected cases, though not with the same beneficial effects. Mr. Syme has expressed his opinion so strongly in favour of the ligature over compression, that he has endeavoured to cast a certain amount of discredit on those who have ably devoted their time and energy to the trial of a more simple and less hazardous cure of this formidable disease. On the revival of compression for the cure of popliteal aneurism, several improvements were made with regard to the manner of applying the pressure, and old notions regarding the duration of time it was necessary to employ were soon eradicated. It was early found that there was no need to apply the pressure so forcibly as to keep the sides of the vessel together, thereby inducing inflammation and subsequent adhesion, which led to the obliteration of the canal; for the operation of Bradsor and Wardrop of placing a ligature on the distal side of the tumour, showed that to obtain a satisfactory result it was not necessary to cut off the entire flow of blood through the tumour; and subsequent experience has proved, that in the treatment of compression it is not requisite so to do, but that gentle and continuous pressure modifying the force of the current, is quite sufficient, in the generality of cases, to cause consolidation within the tumour.

Again, the pressure need not be constantly applied to one particular spot, but may be shifted from place to place along the course of the artery, so as to give the patient as little pain and annoyance as possible.

The result to be hoped for in the treatment of these cases is, coagulation of blood and deposition of fibrin in the aneurismal tumour, which, as I have said, can be produced in many cases by simply modifying the flow of blood through the main artery. The femoral artery is never obliterated at the point where pressure is applied, but can be traced almost down to the tumour itself. If the case go on favourably, and the patient be a proper subject for the plan of treatment, a cure may take place in a few days, or in a week or two; but if, on the other hand, the patient be not content to put up with the inconvenience and pain for a time, the cure may be protracted for a lengthened period. This was well illustrated in a case I had under my care some time ago, when, owing to the extreme irritability of the skin and the condition of the patient, sloughing occurred in the groin, occasioned by the pressure, which greatly retarded the cure.

I do not intend to enter more fully into the merits of the several methods for the cure of aneurism, but shall content myself with stating the treatment I employed in the case under notice, and the results which followed. The aneurism belonged to that class which by Scarpa was termed a "mixed aneurism;" that is, there was probably a dilatation of all the tunics, and then subsequent rupture of the two internal coats.

I employed the instrument that is most in favour among the Irish surgeons, Carté's "circular compressor," in which a very ingenious addition to the common screw force is applied by means of bands of caoutchouc, whereby a certain resiliency is acquired, which keeps up effectual pressure, and at the same time possibly obviates any injurious effects from the screw. Among other contrivances, I may mention the tourniquet of Signoroni; also the one invented by my friend Mr. Bulley, of Reading, which has two screw pads, allowing the pressure to be applied at different spots at various periods. There are other equally ingenious instruments, which possess their individual merits.

At first, I must confess, I did not anticipate a favourable result, as the patient did not appear to be fully aware of the great danger of the disease, and was seemingly very loth to submit to the treatment; but, by explaining to her the importance of resolutely bearing the inconvenience attendant upon such a plan, in all probability a more serious operation would be avoided, so accordingly determined to aid us in the cure; and the following sketch of the treatment shows the satisfactory result:—

I applied the pressure to the common femoral immediately below Poupart's ligament, regulating the force to such a degree as to keep up an amount of pressure just sufficient to stop the pulsation in the tumour. It was continued for twenty hours, when some uneasiness being complained of, it was omitted for an hour or two, and then re-applied. On the third day the pulsation was not so full or throbbing as before. On the ninth day the pressure was modified to such an extent, sufficient to weaken, but not completely to arrest, the flow of blood into the artery. On the fourteenth day, the woman not feeling at all well, all pressure was removed for a few days. On the nineteenth day it was re-adjusted, but, in the evening of that day, the pulsation in the tumour became feeble in a marked degree, and some slight pain was complained of in the ham. On the twenty-first day all pulsation had completely ceased, and the tumour was found diminished in size and of considerable hardness. It was now evident that a cure was effected; and no return of the disease has as yet shown itself.

The most interesting point in the treatment of this case is the rapidity and easiness with which this disease was cured; and it also well illustrates the fact, that it is not always absolutely necessary to make continuous pressure on the artery for any length of time.—*Med. Times and Gazette, Dec.* 30, 1854, *p.* 657.

64.—*On Popliteal Aneurism.*—Mr. Cock placed a ligature on the femoral artery in a case at Guy's Hospital on the 24th ult. in which a large popliteal aneurism appeared on the point of bursting, and in which the mode of cure by compression had not succeeded as well as he had anticipated. It was interesting and instructive to find, as far as London is concerned, that such an operation now at Guy's and several other hospitals is entirely exceptional, the general impression of the surgeons of the larger hospitals being quite in favour of the milder plan of treatment by compression. The man is still under treatment, and doing well; and though a few years ago such operations by the ligature were the rule, and presented little of novelty, the case is of interest as an exception rather to an opposite rule, the plan now generally adopted.

No little controversy has been excited of late years as to what is the best plan of compression in aneurism—a screw, a weight, or a spring. By the almost unanimous consent, however, of all our hospital surgeons, *elastic* pressure seems to be that now generally adopted. In aneurism of any of the arteries of the upper extremities, elastic pressure is more particularly advisable, while a weight of lead or shot may in many cases of popliteal aneurism be found available in controlling the circulation, more particularly at the groin, the screw apparatus having been entirely discarded; the screw tourniquet or clamp, always, amongst other disadvantages, gives rise to impatience of pressure on the part of the patient—he feels as if "pinioned" down or nailed to his bed; while, on the other hand, the elastic spring, if properly placed on the limb, is rather agreeable to the patient than otherwise, and will be found quite sufficient for restraining the current of the blood through the aneurismal sac. Indeed, in a case lately given, a patient continued the spring apparatus of Dr. Carte for seven hours without stopping, at the end of which period the pulsation in the tumour of the aneurism had ceased; the latter had become solid, and nothing else was required to complete the cure, absorption going on rapidly; the screw clamp, on the contrary, in this case, could not be borne even for half an hour. Mr. Teale, of Leeds, lately gave a case cured by compression in twelve days. We have attended with some interest to a case lately treated by Mr. Adams at the London Hospital, and others under Mr. Fergusson.

The statistics of compression are now very satisfactory: of 23 cases of aneurism treated in this manner in London up to last year, 14 had been cured, while in 9 it had partially failed. Very considerable judgment is necessary in selecting cases, and the most vigilant care is required to watch that the few simple directions necessary are properly carried out by the patient; half the cure is in the patient himself. It should not be forgotten, too, that compression, if it do not succeed, as in this case of Mr. Cock's, may do positive harm; if the instrument shift its place off the vessel, the treatment is less than nugatory, as it leads to engorgement of the limb, and diffusion of the contents of the

sac of the aneurism. It is too often said, perhaps, the "clamp" can do no harm; but this is not the case. Besides these 23 cases, it is reported that 47 cases have been treated in Dublin with similar results: 36 were completely cured. These are all the cases from 1842 to 1854, taken indiscriminately.—*Lancet, Feb.* 24, 1855, *p.* 210.

65.—*Treatment of Aneurismal Tumour by Acetate of Lead.* By M. Lassana.—[The diagnosis of aneurism is sometimes obscure, especially when we have to distinguish between an aneurism of the facial artery and a nævus. This subjoined case was one of nævus.]

M. Gelmi, a young girl, aged 22, had from earliest times a soft fluctuation and smooth tumour in the substance of the left cheek, between the commissure of the mouth and the angle of the jaw. It was ovoid in form, and the size of a nut. A puncture gave issue to a jet of blood. In the month of February last M. Lassana punctured the tumour from the mouth with a sharp lancet; again a rapid jet of blood flowed out; but he immediately plunged the canula of a glass syringe, filled with a solution of acetate of lead into the wound; he propelled from the instrument eight or ten drops, and, after having withdrawn it, held his finger applied to the puncture for about a minute. There was no further flow of blood. The tumour, fluctuating and soft as it was before, became solid and hard in less than a minute, and was also somewhat increased in size. No inflammatory phenomena supervened. The swelling diminished by degrees, and the tumour, hard and indolent, became reduced to the volume of a small kernel.—*Annali Univ. di Med.—Med. Times and Gazette, Jan.* 27, 1855, *p.* 93.

66.—*Case of Varicose Aneurism cured by Injection of Iron.* By M. Jobert.—A young man, named Leopold Goëb, aged 18, consulted M. Jobert on the 20th Feb., about a small tumour, situated at the bend of the elbow, which presented all the characters of a varicose aneurism. The tumour disappeared wholly on pressure, but only partially upon compression of the brachial artery. It seemed to be formed by the passage of the blood from the brachial artery into the median basilic vein. Its pulsations were synchronous with those of the heart. It was occasioned by venesection.

Having previously compressed the brachial artery, M. Jobert introduced a trocar into the tumour. Venous blood escaped through the canula. Six drops of a solution of perchloride of iron were injected into the sac through the canula, and the external wound was closed by means of adhesive plaster. Next day a coagulum seemed to have formed in the tumour, the pulsations were less vigorous, and the aneurismal *bruit* less distinctly audible. In a day or two, however, the apparent solidification had disappeared, and the pulsations and

bruit had returned. On the 24th Feb., M. Jobert repeated the injection; and on withdrawing the compression of the artery, a jet of red blood, mixed with the perchloride, was thrown out. Jobert therefore injected the fluid while the artery was compressed. The tumour solidified, and its pulsations ceased; but violent pain was felt in the forearm, and a sensation of heat extended along the course of the radial and ulnar arteries. The muscles of the forearm were spasmodically contracted, and the patient was feverish and sleepless. However, these disagreeable symptoms finally abated, and the tumour continued to become harder; and three days after the operation it was quite solid, resistant, and non-pulsating. The brachial artery had become obliterated for about an inch above and below the tumour, and was converted into a solid fibrous cord.

M. Jobert attributes the failure of the first attempt to the perchloride not having been mixed with a sufficient quantity of blood. The coagulum formed should completely obstruct the artery. He believes the constitutional symptoms that occurred to have resulted from arteritis; hence he thinks this method of cure should only be tried in cases where the aneurismal sacs are of recent formation, and are free from inflammation and disease.—*Gaz. Méd.*—*Monthly Journal of Med.*, *Dec.* 1854, *p.* 557.

67.—ON THE TREATMENT OF VARIX.

By Henry T. Chapman, Esq., late Surgeon of St. George's and St. James's Dispensary.

[Mr. Chapman believes that varix is not cured by obliteration of the diseased vein, and that, moreover, the limb must suffer more or less from this obliteration, besides the necessary risk of phlebitis. Most surgeons seem to found their success on the degree of inflammation to which the vein is subjected, not excepting the recent practice of galvano-puncture. Mr. Chapman says,]

When complete obliteration of the vein, therefore, has happened after galvano-puncture, it is clearly not due to the mere coagulation of its contents, but is the result of acute inflammation producing the consolidation of a considerable portion of the vessel.

I should not have thought it worth while to revert to the rather hasty conclusions drawn from the employment of galvano-puncture, were it not for the probability that similar claims may ere long be advanced in behalf of other agents, such as the perchloride of iron, which also possess the property of coagulating the blood within the vessels. But should any hopes of the kind be entertained, the following observations of Hasse ought to be sufficient to dispel them entirely.

"It is surprising," he remarks, "in how short a space of time a plastic plug, extending through numerous branches, and even

filling a tolerably large trunk, will disappear, so that canals previously impervious are again opened to the circulation." He adds in a note, "This fortunate termination of phlebitis, I have experienced in my own person. The whole system of the saphena, up to its insertion in the crural vein, had become blocked up by plastic lymph. Even the minute twigs of the corium had, by the formation of pustules beneath the epidermis, given proof of active participation in the disease; and yet *within five weeks* of the inflammation subsiding, the circulation was fully restored in almost all the veins as before."

In the modification of ligature by the needle and twisted suture, more recently introduced by Mr. Henry Lee, (*i. e.*, removal of the needle and division of the vein on the second or third day after constriction,) he may possibly attain one of his objects, and prevent absolutely the future restoration of the canal of the divided vessel itself. But granting that it never again becomes pervious, the patient is, in the long run, no gainer by its obliteration, if, as we have seen it explicitly affirmed by Sir B. Brodie, Mr. Hodgson, Mr. Vincent, and others, before many months have elapsed its place is supplied by one or more veins of inferior calibre, of course abnormally dilated, to convey a larger volume of blood than is natural to them. That this is not merely the occasional but the ordinary sequel of obliteration of a venous trunk, whether it take place naturally or artificially will scarcely be disputed. A case I have published elsewhere, will presently be cited, in which the saphena interna was converted into an impervious cord by the insertion of five needles beneath it; and yet within twelve months a varicose vein had been developed, alongside this cord, quite equal in size to the original varix, and occasioning even a greater degree of suffering. In July, 1853, I met with a similar example, but in this second case the relapse occurred in less than four months after the occlusion of the trunk, the operation having been performed in the March previous. Both these patients informed me that they suffered most severely during the process of obliteration, that they were confined to bed for some weeks, and that it was very long before they were able to walk without pain and lameness. Had the veins been divided after ligature, in accordance with Mr. Henry Lee's practice, it is very probable that much of this suffering would have been avoided, and that their recovery would have been incomparably more rapid.

Nevertheless, if the danger of phlebitis is considered by Sir B. Brodie to be a sufficient reason for abandoning his simple subcutaneous section of the vein, Mr. Lee's operation cannot be quite so safe as he appears to regard it. A case, indeed, fully bearing me out in this opinion, which was operated on by him, November 12th, 1852, is referred to in the *Lancet* for March 18th, 1854. Ligature and division of the vein were followed by inflammation, abscess in the leg and groin, and pyogenic fever. On the thirty-eighth day the foot and ankle were still in

a painful œdematous condition; and in addition to these accidents, which so commonly attend the operation, it is recorded that "during the height of the fever, the patient had been seized with severe pain in his right wrist. This pain subsequently diminished, but in the eleventh week again became very acute, the fingers were quite powerless and extended, and the forearm and wrist perfectly useless, though no external ulceration or inflammation had taken place." This occurred in the winter of 1852—3; and the return of the patient to King's College Hospital in the spring of 1854, afforded the reporter of the case an opportunity of ascertaining that, after a lapse of more than a year, the paralysis of the forearm still continued.

By those who still practise the ligature and section of veins for the cure of varix,—and even of varicose ulcers,—it is urged that, in the majority of instances, no serious consequences ensue, and that death from this cause is a comparatively rare accident. But phlebitis, once provoked, is so little under our control, that, until he can say to this disease,—"Thus far shalt thou go and no farther,"—I cannot bring myself to believe that any surgeon is justified in deliberately encountering so formidable a risk, remote as he may deem it, for the sake of an advantage which is at best merely transitory. A single example of such an operation terminating fatally, ought, in my judgment, to outweigh five hundred cases attended by no immediately disastrous results.

I shall now proceed to describe the plan of treating varix, alluded to at the commencement of this paper, which has at any rate the merit of being both safe and simple.

Curative treatment of Varix by wet strapping and bandaging.—One of the writers, I might almost say the only one, who has spoken confidently of the actual curability of varix, is the late Mr. Vincent, whose lengthened experience as Senior Surgeon to St. Bartholomew's Hospital, entitles his opinion to great consideration. At pp. 226 and 228 of his "Observations on some of the points of Surgical practice," he expresses his conviction that veins of the leg, when diseased, will, if the limb is maintained in the horizontal position, and in perfect repose, gradually recover from their thickening and enlargement. "This fact," says Mr. Vincent, "I have observed." Again he remarks, "I have no doubt, that if patients would submit to the confinement, that by means of continued rest, and leeching where this step is required, all cases of varix may be fully relieved."

Since the immediate source of varix is an inability on the part of the coats of the veins to withstand the pressure from within to which they are subjected in the erect posture, the most natural and direct course we can pursue for their relief, is the adoption of measures calculated to diminish that pressure as much as possible. This may, of course, be instantly effected by confining the patient to the recumbent position, as Mr. Vincent has proposed, when all trace of the malady will entirely disappear. But the maintenance of this posture, except-

ing under very aggravated circumstances, is manifestly impracticable. The remedy would be worse than the disease. All that is usually attempted, therefore, is to endeavour to assist the vein in the discharge of its function by giving it support from without, and thus palliating the infirmity.—*Med. Times and Gazette, March* 17, 1855, *p.* 256.

68.—*On Galvano-puncture in the Treatment of Varices and Aneurisms.* By Dr. Werner Steinlein.—The favourable results which have been obtained by the employment of galvano-puncture should induce surgeons to seek for yet surer methods of using it, especially such as expose the patient to less risk of inflammation and suppuration.

Baumgarten and Würtenburg, after the fact announced by Schuh, that coagulation is produced more promptly at the positive than the negative pole, instituted experiments upon the action of the poles taken separately. They obtained the following results:—

1. If the negative pole was introduced alone into a vessel, the positive being applied against the neighbouring parts, there was no coagulation. 2. The two poles introduced into the vessel produced slow, feeble, and rarely complete coagulation. 3. The positive pole, introduced alone, determined rapid and complete coagulation. To turn these results to use, it becomes necessary to inquire. What is the action of the poles upon albumen, fibrin, &c.? M. Steinlein has undertaken this, employing Grove's battery, with poles of platinum, and zinc: the first positive; the second negative. The experiments upon albumen gave the following results:—1. If two conductors of platinum are plunged into a solution of albumen, no coagulation is produced, and test-paper shows no re-action. 2. If the platinum needle of the positive pole be replaced by one of steel, little bubbles are disengaged from the negative pole, forming a thickish layer; the reddened test-paper, applied to the platinum needle, becomes blue; the steel needle at the positive pole becomes slowly covered by coagulated albumen, with no disengagement of bubbles. This needle also becomes bronzed-coloured. 3. If a zinc needle be used instead of one of iron or steel, the coagulation is much more rapid.

It follows from these experiments, that the coagulation of albumen takes place only at the positive pole, and that the coagulating force varies according to the nature of the metal employed. The author then endeavours to explain these results according to physical and chemical laws, and he advises, in acupuncture, that the steel needles should be superseded by those of zinc, or rather by needles covered by a layer of this metal. The galvano-puncture should then be practised in the following manner:—A certain number of needles, connected with the positive pole of a galvanic battery, are inserted into the tumour, whether the sac of an aneurism or the cavity of an aneurismal varix. The negative pole should be supplied with a plate of platinum,

which must be put upon the skin adjacent to the aneurism, after having augmented the conductibility of the epidermis by moistening it with a saline or acidulated solution.—*Zeit. der Gesell.—Med. Times and Gazette, Dec.* 16, 1854, *p.* 620.

69.—ON BENZOIC ACID DISSOLVED IN ALUM AS A NEW HÆMOSTATIC AGENT.

By M. Monsel, and Professor Hannon.

[The following experiments have been tried to test the effect of this agent.]

Application of the Hæmostatic Agent prepared by M. Monsel to the Facial Artery of a Horse.—Experiment 1.—The animal having been shackled and thrown down, the left facial artery was exposed on the inferior maxilla in front of the masseter, and was opened by a longitudinal incision of $4\frac{1}{2}$ lines. The arterial jet having filled several glass tubes, each holding about half an ounce, a sponge soaked in the hæmostatic agent was put upon the wound, and held there by tight compression. The imbibition was often renewed during a quarter of an hour. The hemorrhage recommenced when the compression was relaxed. It being thought that the violent plunges of the animal interfered with the success of the experiment, the sponge was introduced under the integument, which was then tied over it by sutures; the animal was raised, and the head tied high to a manger. From this moment no further bleeding ensued.

The Blood in the Glass Tubes.—Into the tubes, before the addition of the blood, there had been poured half an ounce and ten mimims of both the eau Pagliari and of Monsel's liquid. Coagulation took place equally rapidly in the two sets of vessels.

Employment of the same Fluid on a Dog.—Experiment 2.—Having ascertained on the day following (May 28) that the hemorrhage was definitely arrested in the horse, whose head was still maintained in the same position, a large superficial wound was made in the inner part of the thigh of a middle-sized dog. The solution, applied by means of a sponge, formed a clot in ten minutes; and, by the tight adjustment of a circular bandage, the hemorrhage was definitely arrested.

A few minutes afterwards, a large and deep incision was made into the right thigh of the same dog, and a profuse flow of blood in jets issued from the femoral artery. The hæmostatic agent was applied to the gaping vessel directly by means of a tube; but it failed to arrest the bleeding. Compression was then made at the bend of the thigh upon the vessel, and the soaked sponge, held to the wound, sufficed, as in the former trial, to stop the flow of blood in ten minutes. A tight bandage was applied, and the dog was permitted to rise. The animal licked its wounds, and crouched under the table;

but there was no return of the hemorrhage, as was half expected by those about. The day following (29th) the dog was alive, to the great surprise of all; and it was seen, from the spot where he lay, that no bleeding had taken place during the night. M. Renard then exposed and opened the carotid artery, and treated the wound in a similar way with equal success.

Autopsy of the Dog.—The animal, whose exhaustion was extreme, died, and was examined. The end of the femoral artery (which vessel had been completely cut through) was obliterated by a plug of coagulum, of red colour, and $2\frac{1}{2}$ lines long. In the neck an oval red clot, the size of a nut, was implanted by a pedicle into the carotid artery; this pedicle extended upwards and downwards for some extent within the vessel. The prolongation towards the brain was in the form of a T.

The carotid artery was opened and simply plugged in another dog. Hemorrhage was temporarily arrested by these means; but, owing to the movements of the animal, the wound reopened; the bleeding recommenced, and terminated fatally. Upon examination, no clots were found in the wounded vessels.

Continuation of the Experiment on the Horse.—On the fourth day the dressings were removed, and there was no return of bleeding, no functional disturbance.

June 9. The animal was killed. The facial artery was obliterated, but was found, together with the vein, the nerve, and Steno's duct, imbedded in a quantity of lardaceous matter, the result of the glanders, which had been preceded by farcy. The other morbid changes were, thick white pus in the left frontal and maxillary sinuses; hypertrophy and ulceration of the mucous membrane in those sinuses; ulceration of the pituitary membrane of the same side; miliary albuminoid tubercles in the lungs, the spleen, and the liver.

Employment of the same Liquid on Man.—Hôpital St. André; service of M. Jacquot.—Experiment 4.—Profuse hemorrhage having flowed from the recently cut edges of an old ulcerated bubo in the groin, M. Geoffroy tried in vain to arrest it by means of the ordinary compress. A pledget of charpie, soaked in the hæmostatic liquid by M. Monsel, and placed upon the part, instantly and definitely stopped the bleedings.

Two similar and equally successful trials were made also by M. Renard upon patients in the same Hospital.

M. Monsel is pursuing his investigations still further, with an energy which does him infinite credit. Upon adding ten grains of tannic acid, and a scruple of alum deprived of its iron, to an ounce and a-half of rose-water, he obtained a fluid very efficacious in coagulating the blood issuing from an open vein.—*Gazette Méd.*

Professor Hannon recommends benzoic acid, part one; alum, parts three; ergotin, parts three; water, parts twenty-five; the whole to be boiled thirty minutes, with constant stirring and renewal of water;

then it must be evaporated to the consistence of an extract. The colour is chocolate-brown; the taste strongly astringent; the smell that of ergotin. A layer spread over this bleeding vessel produced instant coagulation. Compression is not necessary.

For *internal hemorrhage* is used, acid benzoic gr. i., alum. pulv., ergotin, aa. gr. iij. Ft. pil. xvi. One pill every two hours.—*Presse Méd.—Med. Times and Gazette, Jan.* 20, 1855, *p.* 64.

ALIMENTARY CANAL.

70.—ON THE RADICAL CURE OF HERNIA BY INJECTIONS OF IODINE.

M. Jobert lately presented to the Academy of Medicine three cases of inguinal hernia in which this new mode of treatment had been productive of the best results.

The first case was that of a young man, 18 years old, with complete inguinal hernia of the left side, which, though it did not interfere with his ordinary avocations, rendered him ineligible for the military service. The tumour did not descend to the bottom of the scrotum, and it was separated from the testis by a constricting band. It was easily reducible by taxis, but reappeared upon the patient coughing or assuming the erect posture.

At the patient's request M. Jobert performed the following operation on the 12th May. An incision, 2 centim. in length, having been made over the inguinal canal, a fine trocar was introduced into the sac, through which 5 grammes of pure tincture of iodine were injected. The wound was then closed by means of twisted sutures, and dressed with simple ointment. The patient suffered almost no pain. In the evening there was slight tumefaction and pain of the inguinal region, but no fever or constitutional disturbance.

May 14. The swelling and redness were considerable. The sutures were removed, and the wound was found imperfectly healed.

In a few days the external wound cicatrised, and the swelling and redness disappeared. The left testicle was not atrophied. On 5th June the patient was able to go about, and to cough without the least inconvenience or reappearance of the tumour. As a precautionary measure he continued to wear a suspensory bandage for some time.

The second case was that of a patient, æt. 34, who entered the Hôtel Dieu on the 18th Nov. 1853, having not only a congenital inguinal hernia, but also a hydrocele of the same side. These were separated from each other by an annular constriction of the tunica vaginalis, which contained the protruded bowel above, and the testicle and spermo fluid below. M. Jobert resolved to obliterate the tunica vaginalis, for the purpose of curing both the hydrocele and the hernia at the same time. He accordingly passed a trocar obliquely into the

lower portion of the constricted sac, and evacuated the serosity therein contained; and then, having previously made pressure with the finger over the inguinal canal to prevent the possibility of there being any communication between the tunica vaginalis and the peritoneum, he injected some tincture of iodine into the tunica vaginalis and the hernial sac. The external wound was finally closed by adhesive plaster. Next day the scrotum was reddened, somewhat tender to touch, and distended with fluid effused from the tunica vaginalis; but there was no pain or constitutional disturbance. On the 5th Dec. the swelling which had been gradually subsiding, had nearly disappeared; and on the 12th the scrotum had regained its normal size. The scrotum and the spermatic cord on the affected side were firmer than before; and a cylindrical cord extended along the whole of the inguinal canal, as well as along the spermatic vessels down to the testicle. The patient was completely cured, and he left the hospital able to walk easily and without fatigue, and without any reappearance of the hernial tumour.

The third case occurred in a young man, aged 27, who had had a large oblique inguinal hernia of the left side, for eight years, for which he had worn all sorts of trusses without benefit. At the desire of the patient, and after a consultation with his colleagues, MM. Rayer, Begin, and Cloquet, M. Jobert punctured the hernial sac, and injected the tincture of iodine into the tunica vaginalis. The patient was laid in the horizontal posture after the operation; his legs were flexed on his thighs, and maintained in this position by means of a bolster. The hernial sac swelled immediately after the injection, and in fifteen hours it was as large as the hernial tumour, and had a fluctuating, half-solid, feeling. The parts continued thus for eight days, and then the swelling speedily diminished. On the twenty-eighth day after the operation the cure was complete; the track of the spermatic cord was occupied by a hard and compact cylindrical substance, which completely prevented any return of the hernial protrusion, and allowed the patient to go about without any recurrence of his malady. Four months had elapsed at the time when M. Jobert related his case, and the cure was still as complete as at first; the inguinal canal was filled with a hard, solid cord; and there was no atrophy of the testicle.

Remarks.—When the sac is adherent to the surrounding parts—when the hernia is congenital—when the sac is distended with fluid, or is thickened or cartilaginous—M. Jobert recommends puncturing the sac at once with a trocar, without previously making an incision. The trocar is to be inserted perpendicularly at first, then obliquely upwards, and, when it has reached a certain depth, it is to be directed cautiously backwards so as to puncture the sac—the accomplishment of which process is known by the operator no longer meeting with resistance, and by his feeling the canula move about without obstruction in the empty sac. Having entered the sac, the pure tincture of iodine should be injected into it by means of a glass syringe—whereby

we may be able to know how much fluid has been used. M. Jobert ordinarily injects one or two spoonfuls *(cuillerées)* of the tincture, but often much less. The quantity injected should always be in proportion to the size of the serous pouch. The injection should be suspended whenever we feel an obstacle to the entrance of the liquid. Instead of allowing the fluid to escape through the canula, M. Jobert withdraws it by means of the suction of the syringe.

When the sac is thin, moveable, and easily displaced, Jobert exposes it by dividing the integuments with a bistoury, and *thereafter* punctures it with the trocar—*never with the knife.* In such cases he unites the lips of the external wound by means of twisted sutures.

What occurs after the operation? Immediately after the injected fluid is withdrawn, a new liquid begins to be thrown out within the hernial sac. After twenty-four or forty-eight hours have elapsed, the sac is filled with this fluid; it continues in this state for eight days, and after the first week it begins to diminish gradually and visibly in bulk. In proportion as the tumour diminishes it begins to acquire solidity, and becomes as firm as wood; nay, it eventually becomes as hard as bone or ivory.

M. Jobert affirms that this method by injection is superior to all the other operative procedures which have yet been devised for the radical cure of hernia, inasmuch as it is at once simple and harmless. M. Jobert, in conclusion, acknowledges that this operation is not invented, but merely revived, by him; inasmuch as M. Velpeau, in his Annales de Chirurgie, published ten years ago, first suggested the likelihood of injections of iodine proving beneficial for the radical cure of reducible hernia.—*Gazette des Hôpitaux.—Monthly Journal of Med., Dec.* 1854, *p.* 555.

71.—*On Strangulated Hernia.*—Mr. Stanley's long experience has taught him the danger of too much handling or manipulating of hernia when incarcerated or strangulated. Indeed, in seven out of ten of the cases sent to the hospital, he is inclined to think one will find omentum, not intestine, as the chief constituent of the tumour, which it is impossible to get back by the rough usage adopted.

In fact, from what Mr. Stanley sees every year, he finds it necessary every season to caution his class against this great error of routine practice, and has adopted the wise maxim of Desault—"think favourably of every hernia if the taxis has not been tried." Mr. Stanley advises to make one fair trial of the taxis when first called in; the little round swelling of femoral hernia is always discoverable in front of the pectinalis muscle. We should relax Poupart's ligament by having the thigh turned up and knee inwards—ten to fifteen minutes will be enough; if pain is present, not even so long. He next recommends a trial of the taxis in a warm bath, and we have seen at St.

Thomas's Hospital, in place of this, the topical application of ice. If either of these fail, he now recommends the *dernier ressort*, chloroform and the knife—one careful trial, but the last, under the anæsthetic, before cutting down. If the parts are gangrenous, he lays much stress on the necessity of disturbing them as little as possible, so as to prevent the contents of the sac getting into the peritoneum. —*Lancet, Feb.* 17, 1855, *p.* 185.

72.—*Hernia treated by the Topical Application of Ice.*—We were particularly struck by some observations lately made by Mr. Clark at St. Thomas's Hospital. He sees some reason to doubt the opinion of Sir Astley Cooper, so generally followed, that the inguinal canal is endowed with muscular contraction, which, under the influence of the abdominal muscles, of course, would close or open it But Mr. Clark agrees, we believe, rather with Scarpa, Pott, Cloquet, &c., that the canal is altogether passive; the chief obstacle to the return of the hernia being its great size. Mr. Clark believes even that tobacco, chloroform, tartar emetic, the warm bath, &c., are not so effectual in reducing an inguinal hernia as the diminution of its size induced by local application of ice, and that the operations on hernia at St. Thomas's Hospital will bear him out in this conclusion, that such medicines and appliances too often protract the cure of the case, or do not act except in a passive or negative form. The topical application of ice, on the other hand, corrugates the tumour, and makes its volume obviously smaller. Mr. Clark seems, curiously enough, an advocate also for opening the sac in femoral and inguinal hernia. He believes, as we have heard him say, that not opening the sac in strangulated hernia is founded on a false idea or analogy—viz., that we can irritate a membrane *more*, already perhaps irritated as much as it will bear. He does not think an unhealthy hernial sac is at all the same as a *healthy* peritoneal membrane, which he should not, of course, wish to wound; and we should never run the risk, he says, of not examining the hernia fully. The question is obviously one of very great practical every-day clinical importance. The opinions of all our various hospital surgeons, however, are not in accordance with those in vogue at St. Thomas's. As far as we can learn, Sir Astley Cooper recomended the use of ice and cold applications to hernia. It was tried in his time chiefly in conjunction with tobacco enemata, the anæsthetic influence of chloroform or ice at that time not being understood. Since then, during the last half dozen years, the use of ice, as a local agent, as an anæsthetic, has given it an importance not at first perhaps suspected. Sir Astley Cooper pounded the ice into rough powder, and placed it in a bladder over the hernia. When ice was not procurable, he mixed equal parts of nitre and sal ammoniac, ten ounces of such salts being added to a pint of water. "If, after four hours," he says, "the symptoms of stran-

gulation become mitigated and the tumour lessens, this remedy may be persevered in for some time longer; but if they continue with unabated violence, and the tumour resist every attempt at reduction, no further trial should be made." This seems a very long time; and a caution is added, not to freeze the scrotum and bring on sloughing.—*Lancet, March* 3, 1855, *p.* 238.

73.—*On the Value of Cough-Impulse as a symptom of Hernia.*—Several instances have recently come under our notice illustrating the value of the knowledge of the fact, that *in tightly strangulated hernia no cough-impulse is ever felt.* In all the reducible or merely incarcerated forms the information derived by making the patient cough is so great and so conclusive, that it is not suprising that attempts should be made to apply the same test to cases in which strangulation exists. Entire forgetfulness of the condition of the bowel could, however, alone induce any expectation that impulses generated within the abdomen would be communicated to it. In all cases in which the constitutional symptoms of strangulation are present, the diagnosis as to whether the tumour be hernial or otherwise must be made without reference to its movements during coughing. Much valuable knowledge on other points may, however, be yet derived from the test. If—in the certainty from other signs that the tumour is really a hernia—there is not the least impulse, a fair inference may be drawn that the stricture is tight. If impulse be felt in the upper part of the neck of the tumour, and not in other parts, the seat of stricture may be accurately determined, being, that is to say, just below the spot where the perception of impulse is lost. On this latter point Mr. Luke has, in his paper "On the Operation for Strangulated Hernia," 'Medical Gazette,' 1839-40, insisted strongly; indeed, we believe he was one of the first to direct attention to its value. With regard to the absence of cough-impulse in strangulated hernia generally, our surgical manuals have, for the most part, neglected to make any observation respecting it, while in some, from the terms used, it might be inferred that it was an ordinary symptom. It is still not infrequent to see cases reported in which its absence is noted as having been a source of difficulty and doubt. Most deeply would it be to be deplored should absence ever lead to delay in performing the operation, since it is, in fact, a conclusive indication of its necessity. We have not made the statement printed in italics above without first instituting much inquiry among those of most experience, and also availing ourselves of every opportunity which has of late occurred for acquiring data in respect to it. The following sentence in its support we quote from a note by Mr. South, in his edition of Chelius:—"I have on more than one occasion heard surgeons of eminence speak of dilatation of a strangulated rupture on coughing, which, I must confess, I think impossible if the rupture be more than incarcerated."—*Med. Times and Gazette, Dec.* 16, 1854, *p.* 616.

74.—*Fistula in Ano.* By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.—The discovery of the uniform existence of an internal opening near the anus may be said to have perfected the operation, but in consequence of the old errors having been so long prevalent, the treatment has not hitherto been nearly so satisfactory as it should have been in the profession at large; and hence the explanation of the fact, that patients affected with fistula in ano, come here from all parts of the country, under the impression that the operation they are about to undergo is a very serious one, and involves long confinement to bed. The operation, though very simple in principle, and easy of performance, is still one that requires care and patience. Whenever you examine a fistula of six weeks or two months' standing, you must proceed on the supposition that an internal opening exists. The track that leads to it may be tortuous, but you must search carefully again and again, if you fail to find the aperture in the first instance, and be very slow to be persuaded that it is not there. A piece of lint is placed in the wound at the time of the operation, and the only other dressing required is washing the part occasionally with soap-and-water for a few days.

The patient, R. M'L., aged twenty-three, a coach-builder from Stirling, was then brought into the theatre, and stated that the disease had commenced as a small suppurating point near the anus, which caused no great pain, and broke of itself, discharging considerably less than a teaspoonful of pus, unmixed with feculant or gaseous matter. The internal opening was very small and inconspicuous, but the internal opening was readily found, a little above the anus, by Mr. Syme, who, in order to demonstrate the existence of the inner aperture, and the small amount of soft parts between it and the surface, brought out the end of probe at the anus, and cut out the probe with the bistoury, remarking that though he had pursued this course in the present instance for the sake of demonstration, yet it was one to be recommended in all cases where much difficulty was experienced in discovering the internal opening, as the most certain means of securing the object in view. A piece of lint was placed in the wound; no hemorrhage whatever occurred after the few drops of blood that were lost during the operation. He was discharged on the 25th December.—*Lancet, Jan.* 20, 1855, *p.* 57.

75.—ON THE TREATMENT OF PROLAPSUS ANI BY STRYCHNIA AND BY THE ACTUAL CAUTERY.

[Mr. Athol Johnson communicates some interesting cases of this affection, which were treated at the Hospital for Sick Children.]

Case 1.—Isabella Addington, aged 2 years, a thin child, of strumous aspect, was admitted May 6, with the rectum protruding about an inch, forming a large, solid, red mass, with the aperture of the bowel

in the centre, the sphincter being much dilated around its base. The child had been subject to prolapsus for some months, but for the last fortnight the bowel is said to have been down almost constantly, his mother being unable or afraid to return it.

By a little gentle manipulation, the gut was replaced; but the sphincter remained relaxed, so that two fingers could easily be introduced; and on the child beginning to cry, the prolapsus recurred immediately. The intestine was again returned, and this time kept up by means of a piece of gutta percha enveloped in lint and secured by a bandage. Mild laxatives, along with tonics, were administered, and a nourishing diet given. The bowel ceased to come down. The sphincter partially regained its tone, and the child was discharged May 16.

On the 3rd of June, she was again admitted. She had evidently been much neglected. The bowel was now protruding about an inch, and had been allowed to remain so for some days. The sphincter was much relaxed. It was now determined to try to produce a more permanent contraction of the muscle. Accordingly, a small blister having been applied to the cleft between the nates, one-twentieth grain of strychnia was applied. No convulsive twitches were noticed; but two hours afterwards, when a motion took place, the bowel did not descend.

On the following day, a fresh prolapsus took place, but not to the same extent as before. On the four day, one-sixteenth grain of strychnia was applied to the side of the anus, the cuticle having previously been removed. Pain was complained off, and the child seemed uneasy; but no muscular contractions were noticed.

Five days afterwards, the bowel still protruding a little after having a motion, one-sixteenth grain strychnia was again applied in the same situation. From this time no further descent of the rectum occurred; the sphincter was more firmly contracted; and on June 30th the patient was discharged, and has not since applied for relief.

In this case the affection was not very severe, and, had the child received a fair amount of attention at home, it is probable that no surgical measures need have been had recourse to; as it was, however, the application of the strychnia certainly seemed attended with benefit; but some rather strong objections attended its use, which will be alluded to at the end of the next case.

Case 2.—Julia Seymour, aged 4, was admitted July 2, with the rectum protruding quite two inches; the mucous surface being inflamed and coated with a muco-purulent discharge. There was said to be great pain on any motion passing, so that her friends had allowed her bowels to be much confined, from their dread of ever giving her any opening medicine. She had been subject to prolapsus for about six months, and for the last six weeks the bowel has scarcely ever been in its natural position. After a little manipulation the gut was replaced; the sphincter, however, remained much relaxed, allowing a

couple of fingers to be introduced through the anus, without any resistance.

Under the influence of rest and laxatives the mucous membrane became more healthy; but the sphincter not regaining its tone, and the bowel constantly descending, on the 6th July one-eighth grain of strychnia was applied over the tip of the coccyx, the cuticle having been previously removed. On the 7th the strychnia was repeated, and again on the 8th. The application of the strychnia produced a good deal of "twitching" about the sphincter, which lasted for upwards of half-an-hour, and, on one occasion, rather severe convulsive action of the muscles of the lower extremities.

Some, but no very decided benefit followed the use of the strychnia, and its application was attended so much inconvenience as to cause its discontinuance.

On the 26th July, the rectum still protruding to a considerable extent every time of a motion being passed, and the aperture of the anus being still in a relaxed state, the patient was rendered insensible by chloroform, and the actual cautery applied in four places, at the junction of the skin with the mucous membrane. The first time of having a motion after the operation the bowel descended slightly, but for some days afterwards it did not come down at all, and the anus remained in a much more contracted state. On the 23rd August, however, there being a little return of the prolapsus, the cautery was again applied, in one spot to the side of the anus.

There was again a slight repetition of the affection on the occasion of administering an injection to bring away some thread worms. On the 29th Sept., there having been no prolapsus for some time, she was discharged as cured.

Remarks.—In the ordinary run of cases of prolapsus ani among the children who apply at the hospital, attention to the state of the bowels, tonics, and astringent injections are usually sufficient. In those children, however, who have been neglected, and in whom the rectum has been suffered to remain down for a considerable time, the aperture of the anus becomes much dilated, the sphincter appears to have lost its tone, and the above-mentioned treatment fails to effect a cure. Is the local application of strychnia of service in these cases? In the comparatively mild forms of the disease, I think that it is of some use, but in the more aggravated cases it cannot be depended upon. Practically, too, I found considerable objections to its use in children. It is necessary first to remove the cuticle, and then to apply the strychnia several times. This proceeding, however, is attended with a certain amount of pain, and it becomes each time more and more difficult to keep the little patient sufficiently quiet to allow of the strychnia being properly administered; either more is used than is desired or even safe, or else, perhaps, in the struggle some is lost, and a sufficient quantity is not employed. Moreover, in practice it is not desirable, if it can be avoided, to have to repeat any

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operation of this kind frequently in young children; the little patient becomes fretful, impatient, and suspicious, the struggles are violent, and almost as much harm is done as good effected.

A more efficacious, as well as a more convenient measure, in these severe forms, would seem to be the application of the actual cautery, in the mode suggested by M. Guersant, of the Hospital for Children at Paris. This operation can be performed easily under chloroform; is, of course, unattended with suffering; and seldom requires to be repeated.

The iron, having a button-shaped point, well heated, is applied to the junction of the skin with the mucous membrane at the verge of the anus, usually in four separate points, to such an extent as to make a pretty decided eschar. It may be mentioned, that even when the patient is insensible, the bowel generally descends with some force during the operation, so that it is necessary to be prepared with a piece of sponge or lint to protect the protruded gut while the iron is being applied to other points. Scarcely any pain is complained of afterwards, the child being ready to play and laugh almost as soon as it recovers from the effects of the chloroform. Simple dressings may be applied to the resulting sores, which usually heal without much trouble or difficulty.

With regard to the *modus operandi* of this operation I am inclined to think, from the immediate benefit produced, that it acts, to some extent, at any rate, by the powerful stimulus to the sphincter muscle, increasing its tone, rather than by the resistance afforded to the descent of the gut by the contraction of any cicatrix which may subsequently be produced.

From the little experience I have already had, I certainly prefer this operation to the application of strychnia in those cases of prolapsus ani in which any surgical measures are required.—*Med. Times and Gazette, Nov.* 18, 1854, *p.* 521.

76.—*Prolapsus Ani treated with Nitrate of Silver.*—[Mr. Lloyd, of St. Bartholomew's Hospital, has succeeded in curing many of these tiresome cases.]

The plan is to smear the whole surface of the protruded bowel with the solid caustic, and then return it. The application is repeated once in a week or fortnight, as may be requisite. Mr. Lloyd informed us that he rarely found it necessary to employ it more than three or four times; and further, that although the plan had been one invariable resort with him, for a long series of years, that he had never known any untoward consequences to result. In cases in which the protruded bowel has become swollen, and is difficult of reduction, the effect of the caustic is surprising. In one such case, in which we witnessed its application, the mass could be easily seen to diminish in size under its influence. Mr. Lloyd does not limit the use of this remedy

solely to prolapsus, but adopts it also in cases of hemorrhoidal congestion, and thickening of the mucous membrane about the verge of the anus.—*Med. Times and Gazette, Feb.* 10, 1855, *p.* 136.

77.—*Treatment of Piles.*—The following is the formula of an ointment often found very useful as an application to piles, when not of very large size. It is copied from the Pharmacopœia of Guy's Hospital:—

℞. Gallarum contrit. ʒij.; opii (emolliti aquæ cum ʒj.) ʒss.; liq. plumbi diacet. ʒij.; adipis ʒj. Misce. Ft. ung.

We have here combined the astringent properties of lead and of galls with the sedative and anodyne ones of opium. Of course, in having resort to local measures, the general ones which address themselves to the cause of the disease should never be neglected. On the use of mercurials and castor-oil, as the radical treatment of hemorrhoids, we had occasion to comment a few weeks ago. There is no no doubt, however, that the sufferings of the patient may be materially mitigated by topical applications. If the piles be large, ulcerated, and very painful, the free application of nitric acid will be the best; if in a more chronic condition, the above ointment may be ordered, or lunar caustic may be freely smeared over the parts. A good formula for an astringent ointment is valuable, even if the other means be in a general way preferred, since, in private practice, cases are not unfrequently met with in which the surgeon must prescribe without being permitted to inspect the part.—*Med. Times and Gazette, April* 14, 1855, *p.* 362.

78.—*On Hemorrhage from Piles.* By M. Trousseau.—A woman, aged 38, pale, anæmic, and very weak from loss of blood from the rectum, entered M. Trousseau's wards. On examination the hemorrhage was found to depend upon piles. She was ordered injections of nitras argenti of various degrees of strength—from 20 to 40 centigrammes of the salt, without any benefit. Enemata of rhatany were next tried, but also unsuccessfully. M. Trousseau therefore ordered an injection of 50 centigrammes of sulph. cupri, dissolved in a glassful of water, to be administered daily. The hemorrhage gradually became less, and in eight days had quite disappeared. The patient was ordered chalybeates internally.—*Gaz. des Hopitaux.—Monthly Journ. of Med., Dec.* 1854, *p.* 562.

URINARY ORGANS.

79.—ON STRICTURE OF THE URETHRA.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[Mr. Syme's operations have now been sufficiently long before the public to be tested as to the permanency of the cures. The following case will show for itself.]

J. R.. aged forty-four, a fisherman, from Prestonpans, was admitted into the Hospital on the 17th of March, 1852, on account of a stricture in the urethra of sixteen years' standing. Previously to admission he had been treated by bougies on various occasions, and under this mode of treatment had always suffered severely from rigors and febrile symptoms, without experiencing any benefit of longer duration than the period of dilatation. Latterly his symptoms had become more aggravated, and every attempt to pass instruments had proved ineffectual. A full-sized instrument was arrested at the bulb, and, not without considerable difficulty, a No. 1 bougie was introduced into the bladder. The introduction of this instrument was followed by a severe rigor, and next day the patient suffered from retention of urine, to relieve which a No. 1 catheter was introduced. Dr. Mackenzie made repeated attempts to continue the treatment by bougies, but the constitutional disturbance produced by them was so severe, that he determined without further delay to divide the stricture. Accordingly, having introduced a grooved staff, the size of a No. 1 bougie, into the bladder, he made an incision in the middle line of the perineum, over the induration at the seat of stricture, and divided the contracted portion of the urethra to the extent of about three-quarters of an inch. A No. 8 catheter was passed with great facility into the bladder, and retained there, not more than half an ounce of blood being lost either during or after the operation. There was subsequently a slight degree of febrile disturbance, which subsided when the catheter was removed, at the end of forty-eight hours. During the following ten days, the urine passed in nearly equal proportions by the urethra and the wound, but before the end of three weeks had entirely resumed the natural channel. He remained in the hospital a few weeks longer, during which a full-sized bougie was passed occasionally without the slightest difficulty, and was dismissed on the 12th of June. He was seen again on the 9th of June, 1853, when a No. 12 bougie was passed with ease; and the patient stated that, although he had been much exposed to cold and wet in deep-sea fishing, he had never experienced the slightest difficulty of micturition. In April, 1854, he applied for admission to the hospital on account of symptoms supposed to be rheumatic, but which were traced to aneurism of the innominata. He suffered from no urinary symptoms whatever, but was admitted on account of the aneurism, and died in the beginning of June.

The symphysis pubis, bladder, and urethra, are now before you, and the symphysis has been divided, so as to allow the urethra and neck of the bladder to be laid open from above. A white depressed line of cicatrix, on the inner surface of the urethra, extending, in the mesial line, half an inch forward from the extremity of the bulb, corresponds exactly with the line of cicatrix in the integuments and intermediate textures. The urethra in this part is rather wider than natural, from, having a slight funnel-shaped depression on the lower surface, and before being opened, easily admitted a No. 12 bougie. In other respects

the coats of the urethra present a perfectly natural aspect, and the spongy tissue of the bulb, as well as the neighbouring parts, are free from induration. There are two false passages in the prostate, evidently of very old standing. They are both seated at the upper surface of the urethra, one of them commencing at the triangular ligament, and forming a cul de sac in the prostate; the other, confined to the gland. The bladder, with the exception of being a little thicker than natural, presents nothing remarkable.

No one, looking at the urethra, which has remained thus sound and ample at the part where it had been so long and so tightly contracted, although two years had elapsed since the operation without any means being used to prevent relapse, can entertain a reasonable doubt as to the recovery proving permanent, however long the patient might have lived; and such a result should stimulate our exertions to discover, so that they may be avoided, the errors of performance to which any recurrence of the symptoms that may have happened should be attributed, rather than to the principle of the operation.

But the parts now before you afford a no less instructive lesson in regard to another most important point in the treatment of stricture. This is, the position of the disease, respecting which the most vague and conflicting opinions still exist in the profession, as you may judge from one of the most esteemed authorities, Sir B. Brodie, representing the membranous portion of the urethra as the most common seat of contraction, while I am prepared to prove that it never occurs in this part of the canal. The erroneous opinions which exist on the subject have proceeded from two sources. Of these, the first is the difficulty of discriminating between the resistance to passing bougies into the bladder which is caused by the contracted part of the canal grasping the instrument that has been carried through it, from an obstruction to the progress of the point; while the second is the deceitful feeling of a false passage, which allows the introduction, without much force, so far as the surrounding textures are soft and yielding, but cannot be extended beyond any firm texture, or still less permit an entrance into the bladder. The truth is, that if you divide the urethra into three portions, of prostatic, membranous, and the remaining part, which is covered by the corpus spongiosum, it is to the last alone that all strictures may be referred. However completely any one might be satisfied in his own mind as to this, he would probably find it impossible to convey the same persuasion to others, so long as he merely appealed to the practicability of passing instruments through the strictures alleged to exist in the posterior part of the canal, because success in doing so might be attributed to chance or good fortune on the part of the operator. But having now had occasion to operate more than a hundred times upon strictures of every sort and degree, by cutting externally upon a grooved director, so that the precise seat and extent of the incision could be determined, and having never been prevented from passing a full-sized instrument into the bladder, although I have never

cut beyond the bulbous part of the urethra, I am entitled to conclude, and without any qualification to affirm, that the fact is as has just been stated. Now, if you look at the prostatic portion of this urethra, you will see that various false passages have been made into its substance, so as to establish a cul-de-sac, which must plainly have resulted from the pressure of an instrument; and there can be as little doubt, that when the operators found their progress obstructed here, they believed that they had to deal with an impermeable stricture at the neck of the bladder. But you see that Dr. Mackenzie did not cut further back than the bulb, that he was nevertheless able to pass, without difficulty, a full-sized catheter into the bladder, and that the patient was completely relieved from his symptoms. There is at present under my charge a gentleman who has come from England, having been from home nearly two years, under the treatment of an eminent practitioner, who, he says, introduced instruments, with few exceptions, every day, for the remedy of a stricture which, he said, was seated at the verumontanum. You will now be able to understand the explanation of such a case.

This operation for stricture has passed through the period of opposition, and is now exposed to the danger of being adopted by persons who certainly do not, and either cannot or will not, understand the principles upon which it is founded. These principles may be said to be—

First—Stricture is never seated posteriorly to the bulb, and therefore the incision should never extend further back than the bulb.

Second.—A grooved director must be insinuated through the contracted part, without injury or abrasion of the lining membrane of the urethra.

Third.—If the incision has been properly performed, there is no need to dilate sinuses, which are sure to close when the stricture is removed.

Fourth.—A catheter should be introduced into the bladder after the operation, and retained for forty-eight hours; not less, on account of the risk of extravasation of urine; and not longer, because it is unnecessary, and apt to do harm.

You may fancy with what astonishment I read in the journals from week to week, statements to the effect that strictures are very often seated in the membranous part of the urethra, and that incisions are therefore required there, or even at the neck of the bladder. I also find it stated that in cases of impermeable stricture the bougie must be pushed onwards by force in the direction which the anatomical knowledge of the surgeon leads him to suppose to be the course of the urethra. In a journal published the other day, a case is recorded, in which the operator is said to have divided a stricture upon a silver catheter. Consider for one moment the danger of such a proceeding; even if the urethra were healthy there would be great danger of

wounding the artery of the bulb if the knife should glide off the catheter; but considering the induration of the parts divided, and the uncertainty of the precise seat of contraction, I do not hesitate to assert that it is impossible to divide a stricture with certainty or safety in this way. For my own part, I would as soon think of dancing on a tight rope as of making such an attempt. It is further reported that in this case a number of sinuses were laid open, and that a catheter was retained in the bladder for eleven days, and yet it is said by the reporter that this operation was conducted emphatically after my manner. I say emphatically that it was not conducted after my manner, but was a miserable travesty of it, which would be simply ludicrous were it not that its result was so tragical.

[The patient having been brought into the theatre, and chloroform administered, Mr. Syme passed a No. 2 bougie into the bladder, and afterwards introduced a No. 9 bougie for a short distance into the urethra, to make sure that a stricture near the orifice had been dilated sufficiently to admit the thick part of his new staff. This is a steel instrument of the same general form as a catheter of short curve, with the curved part of the size of a No. 1 or 2 bougie, and deeply grooved on the convexity. The rest of the instrument up to the handle is of the size of the No. 9 bougie, and destitute of a groove. The groove in the thin part terminates anteriorly in a notch in the abrupt commencement of the thick part. Having introduced this instrument through the stricture, he made an incision, about two inches long, in the raphé of the perineum, and cut carefully down in the middle line, through the mass of indurated textures, till he felt the staff with the forefinger of his left hand. Taking the staff into his own hand, he introduced the point of the knife into the groove, about an inch and a quarter behind the commencement of the thick part, holding it with the handle upwards, and the back supported in its entire length on the forefinger, which he was thus able to use to feel where the knife was going, while it removed all risk of the point of the instrument slipping deeply on either side of the staff. Having depressed the handle, he then pushed the knife forwards along the groove, through the tough substance of the stricture till the point was arrested by the notch at the commencement of the thick part of the staff. To ensure the complete division of the anterior part of the stricture, he now pushed forward the staff and knife together for a short distance. He then introduced into the bladder a No. 11 catheter, placing the left index finger in the wound as a guide. This was necessary on account of the great depth of the incision through the indurated perineum, which measured full two inches. The catheter was provided with a stop-cock, and the end curved downwards to prevent the inconvenience of the trickling of urine upon the body. Everything went on favourably, the urine being discharged from the wound entirely for a week, and then gradually resuming its natural course, so that a fortnight after the operation, a few drops only escaped by the wound, the

swelling of the perineum had entirely disappeared, he no longer suffered from incontinence, and passed his urine in a good stream. The operation was performed on the 27th of November, and the patient was discharged fit for duty on the 16th of December.]—*Lancet, Jan.* 13, 1855, *p.* 27.

[Mr. Syme brings forward the following interesting case, on which he makes some very important remarks. The patient was aged 37, a seaman, treated by dilatation eight years ago.]

At present, micturition is very laborious and frequent, sometimes hourly, and the stream of urine extremely slender. On examination, I found a bougie of moderate size arrested, an inch and a half from the orifice, by an obstruction, which could not be passed by the smallest bougie; I then tried a common probe, but this also could not be passed. I now had recourse to the instrument which I hold in my hand, which was made for cases of this description. You see it is considerably finer than a common probe at its extremity, and gradually increases in thickness towards the handle. This instrument passed without the slightest difficulty. If the stricture had been seated at the bulb, it might have been said that the fact of my being able to pass only so small a instrument was no certain evidence of an extreme degree of contraction, as there is difficulty in guiding the instrument with precision at that part of the canal; here, however, there is no such source of fallacy, for we can direct the instrument with certainty in the course of the urethra, and, small as it is, it is firmly grasped. The first remark I would make regarding this case has reference to what is called impermeability of strictures. Now, the No. 1 of our set of bougies is the smallest size that is made, while many sets have none so small; so that this stricture would be impermeable to all ordinary bougies. On the other hand, the very small instrument I have just shown you, passed without the slightest difficulty; and this case, therefore, furnishes a good example of the principle that all strictures that allow urine to pass out, will admit of an instrument, sufficiently small, being insinuated through them. The next point to which I would direct your attention, is the situation of this stricture, anterior to the scrotum. It is here that the tightest strictures have been observed to occur, which is fortunate, for if there should be a stricture of the same degree of tightness behind the scrotum, its remedy would be attended with extreme difficulty. In the present case, I feel the induration of the stricture like a small pea, somewhat elongated. Now, experience shows that of all strictures of the urethra, this condition is the most unmanageable by bougies, the patient tells us that so far from having experienced benefit from the introduction of instruments, his symptoms have rather become more aggravated, and I believe we might continue dilatation for any length of time without advantage. Instead, therefore, of pursuing this unsatisfactory course, I propose to divide the stricture at once. But you

will say, How can it be divided? We must use a proportionately small director, the employment of which would be attended with great difficulty at the bulb, but will be very easy here. In this part of the canal there is no occasion for a long incision in the skin; the point of the knife being entered into the groove of the director, anterior to the stricture, will be pushed backwards through it, while the end of the penis is held firmly; the same principle being followed in cutting backwards here, as in cutting forwards for a stricture at at the bulb—viz., that of proceeding from the side where there is greatest resistance. After this, I shall pass a full-sized instrument, and I expect it will go freely into the bladder. Of this we cannot be certain till the stricture at the anterior part of the canal has been divided; but, as far as I remember, I never met with a tight stricture behind the scrotum along with an extreme degree of contraction anterior to it. There will be no need to retain a catheter in the bladder; the urine will flow freely, and there will be no risk of extravasation; even if it did occur to the small extent which is alone possible, I should regard it as a favourable circumstance, on account of the effect it would have in preventing the urethra from uniting by first intention.

[The patient was now brought into the theatre, and Mr. Syme, having shown that the No. 1 bougie was arrested an inch and a half from the orifice, while the delicate instrument above described passed the obstruction without difficulty, endeavoured to introduce a very fine grooved steel director. This, however, could not be passed beyond a certain distance; but Mr. Syme, on feeling the instrument through the tissues of the penis, found that the point of the director had passed the stricture, which he felt like a pea upon it, and that it was only in consequence of the instrument being tightly grasped that it refused to pass further. He, therefore, proceeded to operate as he had proposed, after which he introduced successively three different sized bougies through the seat of stricture, and pushed on the largest (No. 8) with ease into the bladder, thus proving that no obstruction existed behind the one divided. Scarcely a drop of blood escaped during the operation. Mr. Syme then made the following remarks:—]

This, then, is, as you have seen, a very simple process; yet the patient may be almost said to be entirely cured. I think it likely that, instead of leaving the hospital very imperfectly relieved, after several months of dilatation, he will in a few days go away, without requiring any further precaution than the occasional introduction by himself of a steel bougie, two inches in length, to prevent the risk of contraction during the healing process. You will, I trust, not soon forget that remarkable fact which you have just witnessed—viz., that, after I had introduced the very small director fairly through the stricture, the firm grasp of the contracted part made it impossible to

push it further without a degree of force, which I did not feel justified in using.

[The operation was performed on the 15th of January. On the night after the operation the patient retained his urine for seven hours, and never afterwards voided it with unnatural frequency. It flowed in a full stream by the urethra, but for a few days issued also to a small extent by the wound, in the neighbourhood of which the cellular substance showed some signs of irritation by swelling and tenderness. Full-sized bougies were occasionally introduced, and he left the hospital to join his ship on the 6th of February.

In his lecture on the 25th of January Mr. Syme said ;—]

There are two patients who have been in the hospital for the last seven weeks, with stricture of the urethra at the bulb, as tight as you ever met with in that situation. It was at first difficult to say which of these cases was the worse; both had extreme difficulty of micturition, and both incontinence of urine, and both required, in the first instance, the smallest-sized bougies, which I experienced some difficulty in introducing. By passing instruments regularly at intervals of three or four days, I was soon able to introduce a large sized bougie in both cases, but neither appeared at first to improve in their symptoms under the dilatation. By and by, however, one of them did derive benefit; he lost the incontinence, and could pass his urine in a much better stream—in short, he is now so much improved, that I think in his case we might be satisfied with dilatation. You observe I express myself with some hesitation; for the more I see of these complaints in this obstinate form, the more doubt I entertain as to the expediency of trusting to dilatation for their cure. The other patient, however, has not experienced the relief that we desired; his difficulty of micturition is almost as great as ever, and his clothes are still wet both day and night from incontinence of urine. I also found after leaving him for a few days longer than usual without the introduction of instruments, that, instead of going on with the large-sized bougie I had used on the last occasion, I was obliged to begin again with No. 1. On my next examination of the urethra, ten days later, I was unable to pass even the smallest size. In this case, therefore, if we had no means except dilatation, we should be puzzled and at a loss what to do next; but I need hardly say that we shall feel no such perplexity, but shall proceed to the other mode of relief for stricture—viz., dividing it by external incision. But before having the patient brought in, I will mention to you a striking case, at present under my care in private. During my short residence in London this gentleman came up from Aberdeen, and placed himself under my care, in order to obtain relief from a stricture, which had been treated for some time as impermeable by several surgeons. I at once succeeded in introducing a small instrument, and, after employing dilatation for three weeks, the urethra admitted a full-sized bougie, and he went

away apparently well, and with the hope of continuing free from his symptoms, with the occasional introduction of an instrument. Instead of this, however, he has ever since—that is to say, for the last seven years—been perpetually under the care of surgeons in different parts of Great Britain. On one occasion he had complete retention, and several surgeons then failed to pass a catheter. But the remarkable circumstance is, that he has been during this period repeatedly treated by dilatation to the full extent. And now comes the most important point of all:—this patient, though in full general health, not above forty years of age, married, and leading a regular life, and therefore not in danger of contracting the disease afresh, yet, even when the stricture was fully dilated, has never passed urine with ease; he has even felt an insecurity of being able to void it at all, and has been frequently obliged to go out of church or to leave company in consequence of the importunate desire for micturition which has accompanied his complaint. His life has, in short, been miserable, and this, you observe, at times when bougies of full size could be passed into the bladder. This is an example of that form of the disease which first led me to aim at some other means of effecting that for which dilatation was evidently inadequate. These cases were formerly the opprobria of surgery. It is only three days since I divided the stricture in this gentleman; yet he told me yesterday (I have not seen him to-day) that he felt as if something was taken away that had made life miserable; an incubus had been removed that had been weighing him down during many long years of suffering. (The relief in this case proved permanent.)

[The patient was now brought into the theatre, and chloroform having been administered, Mr. Syme succeeded, after some little difficulty, in passing the thin part of his staff through the stricture. The rest of the operation was easily completed in the usual way. Mr. Syme, while he had his finger in the wound, observed that he could feel the artery of the bulb beating at the side, and remarked that this showed forcibly the danger of deviating from the middle line in making the deep part of the incision. Mr. Syme then easily introduced into the bladder a No. 11 catheter provided with a stop-cock. The patient having been removed, Mr. Syme made the following remarks:—]

You had, not long ago, an opportunity of seeing the operation performed in a case complicated with fistulas, and great thickening and induration of the perineum; those are the cases in which the operation presents the greatest difficulty. Again, ten days ago, you saw me divide a stricture anterior to the scrotum, in which the operation presents the greatest simplicity, and is easiest of performance. That patient, I may remark, continues well, passing his urine without pain in a good stream, and the urethra admitting a full-sized instrument. The operation you have just seen holds a middle place as regards difficulty of the operation, the stricture being seated as far back as it is

ever met with, but the perineum free from induration. You see here on the table the blood that was lost during the operation; though he is a young man of thirty, in full health, and with rather greater disposition to bleed than is usual, yet two teaspoonfuls would be a liberal allowance for the amount of the hemorrhage.

[Everything afterwards went on favourably; on the removal of the catheter at the end of forty-eight hours, the urine passed in a perfectly full stream by the urethra, and continued to do so, none having escaped by the wound. In a few days he was going about the ward, and might then have been dismissed, but remained under observation until the 27th of February, nearly five weeks after the operation, when he got his discharge after a full-sized bougie had been passed with perfect facility into the bladder.]—*Lancet, March* 10, 1855, *p.* 254.

80.—STRICTURE OF THE URETHRA OF TWENTY YEARS STANDING.

By Henry Lee, Esq., King's College Hospital.

This patient is a hair weaver, about forty years of age, of a thin, sickly look, and observed the first symptom of stricture twenty years before his present admission. At that period retention of urine suddenly came on, when a personal friend of the patient, seeing the state he was in, shaped a piece of *whalebone from an umbrella* into the form of a bougie, and passed it up the urethra. Immediately after the operation, the urine flowed copiously.

The patient now remained well for three or four years, when symptoms of stricture again presented themselves, but not to such a degree as to cause him much inconvenience, the use of the catheter being, however, now and then required.

Four years before his admission here, the distress from the stricture became suddenly so great as to induce Mr. Guthrie, under whose care he then was, to divide the stricture with Stafford's instrument. After this operation, the patient passed his urine freely, for a short time, but soon the stricture returned, and he has been suffering from it more or less up to his admission. When first seen in the hospital, Sept. 14th, 1854, there was pain in the lower part of the abdomen, and across the loins, and the bladder could be distinctly felt, through the parietes of the abdomen, to be hard and full. There was total inability to pass urine, which fluid only came in drops. The endeavours to introduce a catheter, both without and within the hospital, having proved useless, Mr. Lee had the patient brought into the theatre, passed down an instrument as far as the obstacle, and with no better guide than the end of the catheter, freely divided the urethra. When this perineal section was accomplished, No. 6 catheter was introduced into the bladder, and there retained for five days.

The urine a few days afterwards passed both through the wound and

the urethra, and the patient after remaining several weeks in the hospital is on the point of being discharged. The wound is all but healed, and scarcely any urine escapes through the perineum.

Mr. Lee, in a clinical lecture on this case, after giving a sketch of its principal features, and having pointed out the different kinds of unpleasant consequences following stricture, and the various methods which have been devised for the cure of contraction, said—

"The plan of dividing stricture by instruments passed down the urethra has been variously modified; sometimes an instrument of the shape of a trocar has been made to protrude from the end of a catheter, so as to perforate the stricture; sometimes the instrument used has been made in the shape of a lancet, and sometimes a long very thin knife has been passed down to the stricture upon a director. Another plan has lately been adopted in France where an instrument could be introduced into the bladder. The instrument used resembles in construction a lithotrite, but the blades are capable of being separated from each other to the extent of about a quarter of an inch. The instrument is then introduced, and the blades suddenly separated, so as forcibly to tear open the stricture.

Now all these plans have the disadvantage, besides that of endangering the vitality of a portion of the mucous membrane of the canal, of not providing an escape for any portion of urine which may flow from the urethra through the opening which they make. Under these circumstances it has been proposed to make an incision in the perineum, and to divide the stricture from without. This plan, of which I now propose to speak, has at least the advantage, when properly performed, of being free from the dangers arising from making false passages, extravasation of urine, and purulent deposits.

The cases for which this operation has been recommended are of three kinds:—

1. Where the stricture presents an extreme degree of irritability, and resents, by violent local and constitutional disturbance, any efforts to produce dilatation.

2. Cases in which the stricture, when dilated, rapidly contracts again.

3. Cases in which, after the dilatation of the passage, micturition is nevertheless painful, difficult, and uncertain.

To these three classes we may add a fourth, not admitted by some surgeons—namely, cases in which no instrument can be passed into the bladder.

For these affections a free incision of the contracted part of the urethra has been maintained to be the proper mode of treatment, and to be in fact required.

The mode of performing this operation, as described by Mr. Syme, is as follows:—

A grooved director is first introduced through the stricture, where this can be done, (and Mr. Syme is of opinion that, with care and attention, there is no stricture through which an instrument may not

be made to pass.) The patient then being placed upon his back, at the edge of the table, with his legs bent, as in the operation for lithotomy, an incision, about one inch and a half in length, is made exactly in the raphé of the perineum. The whole of the thickened, indurated, and contracted texture is then divided upon the director, to the extent of an inch or two, or more if necessary. A No. 8 silver catheter is then passed into the bladder, and allowed to remain there for at least two, and not more than three days.

Great stress is laid upon the fact of the incision being made exactly in the middle line of the perineum, in order to avoid the artery of the bulb which lies upon the side of the canal.

The only sources of danger alleged to exist are, hemorrhage and extravasation of urine. But if the knife is kept exactly in the median line, the only vessels that are likely to bleed are, the smallest branches of the superficial perineal artery, and the cells of the corpus spongiosum; the bleeding from these may be checked, should it be desirable, by placing a piece of folded lint between the edges of the wound, and applying the slightest degree of pressure for a few hours.

The liability to extravasation of urine after this operation, I will presently consider, reserving it for separate remarks, as I conceive the liability or otherwise to its occurrence forms the grand distinction as to this operation being admissible or not.

After the operation of perineal section, the catheter is tied in the bladder, and the patient put to bed. At the end of 48 hours the catheter may be removed. A full sized bougie should be introduced once a week for three or four weeks, and then at more distant intervals, according to circumstances.

Now this operation, as I have described it, appears a very simple affair, and if this were all, there is little doubt which is to be preferred, the pain and inconvenience, (to say nothing of the danger of a stricture) or the simple operation of dividing a portion of the urethra on a grooved director. But, unfortunately, the operation has not been found to be one of such a very simple nature in its consequences, even where an instrument could be got into the bladder, much less in cases where no staff could be passed.

Mr. Syme, of Edinburgh, asserts that he has performed this operation a great number of times, without any ill effects, excepting only some consequent symptoms of nervous irritation. But in other hands the most serious mischief has often supervened. Patients have been attacked with a shivering fit the day after the operation; there has been a quick, irritable condition of the pulse, accompanied perhaps by profuse perspiration, and want of sleep. These symptoms have continued, the patient's tongue becoming brown and coated, and, in a certain number of cases, the patient has died. On a post mortem examination, some purulent infiltration has generally been found about the neck of the bladder, or some secondary inflammation in other parts.

Now how are we to account for such different results in the practice of various surgeons of equal skill? I believe that something like a solution may be arrived at by an attentive consideration of the description which Mr. Syme has given of his operation.

In the way in which he performs it, the only fascia concerned, as he says, is that which lies immediately under the integuments. In other words, he divides only the skin, superficial fascia, and the urethra. Now it is clear from this description, that all the strictures which he has operated upon have been situated anterior to the membranous portion of the urethra; for had he operated upon any stricture in the membranous portion of the canal, even though situated quite at its anterior part, he must have been in danger of wounding the deep perineal fascia as well as the superficial. When a stricture is situated in the bulb of the urethra, it may be divided from without, and any urine which escapes from the passage is sure to pass out at the external wound. But the circumstances are different when the knife, in passing along the grooved director or sound, perforates the deep perineal fascia, and wounds the urethra as it passes through this part. The urine which escapes from the passage may then lodge in the wound made in the deep perineal fascia, and a drop or two may become infiltrated behind this dense structure. It will there give rise to inflammation, and having no means of escape, will produce violent constitutional irritation. When once inflammation is established in the cellular tissue of this part, its products will permeate the areolar tissue, and may thus propagate the inflammation to the outside of the bladder, and to the cellular tissue within the pelvis, thus giving rise to the abscesses and the purulent infiltration which I have mentioned.

But where the incision is confined to the superficial fascia, and to the bulb of the urethra, there is, as I have said, very little danger of any of these accidents occurring; and by choosing his cases, (as it were,) and confining himself to those in which the stricture is situated anterior to the membranous part of the urethra, Mr. Syme has met with the success to which I have alluded.

Hence there appears to be two classes of cases: those in which the stricture is situated in that part of the urethra corresponding to the corpus spongiosum, and those in which the stricture is at the anterior part of the membranous portion of the urethra. In the former situation, as far as our present evidence goes, the stricture may be divided with comparative impunity; in the latter, most severe and even fatal symptoms have followed.

But practically, it will be asked, how are we to know that a stricture is confined to the bulb of the urethra, or to any part in front of this? Where an instrument can be introduced, the point may be made out in this way, and Mr. Syme never operates unless he is able first to introduce an instrument into the bladder.

In the case to which I have drawn your attention, I was led to believe that I might with impunity divide the stricture, from the circum-

stance of Mr. Guthrie having divided it by internal incision before, without any ill effects. This proued to me, either that the stricture was confined to the bulb of the urethra, or, if situated further back, that the surrounding parts were so consolidated by inflammation as to preclude the danger which might arise from infiltration of urine.—*Lancet, Nov.* 25, 1854, *p.* 436.

81.—*New Instruments for Retention of Urine and for Fistula.*—Some cases of retention of urine from obstinate stricture, requiring puncture through the rectum, have ended fatally, lately, in hospitals. The instrument unfortunately slipped out, but was obliged to be replaced. It has been suggested to try an instrument which would not slip out, and one of this description is now used at Guy's, a modification of Mr. Wakley's, which expands by a spring when once in the bladder.—Mr. Partridge lately operating for fistula, used a very ingenious probe-pointed bistoury, with "movable sheath," made of silver, which slides off to any extent when the instrument is introduced, thus offering a cutting edge of greater or less extent, according to circumstances.—*Lancet, Dec.* 23, 1854, *p.* 528.

82.—*Nocturnal Seminal Emissions treated by Digitaline.* By M. Laroche.—Digitaline, of late much extolled and employed with success, according to many practitioners, in cases of nocturnal seminal losses, has yielded, in the hands of Dr. Laroche, a result which deserves to be mentioned. A young man, aged 18, presented himself some months ago to be treated for nocturnal seminal losses. The disease had come upon him about two months previously. For twenty days, the pollutions had not omitted a single night; his strength was gone, the appetite had left him, and his sleep was troubled by unpleasant dreams. M. Laroche prescribed three granules of digitaline (equal to a grain of the powder of the plant). The night following the administration of the medicine the pollutions intermitted for the first time. In three weeks he was cured.—*Med. Times and Gazette, Nov.* 4, 1854, *p.* 473.

83.—*Incontinence of Urine in the Old.* By John Z. Lawrence, Esq., Surgeon to the Farringdon Dispensary.—[In a case of this description in an old man, owing probably to some want of power in the bladder, and not to diseased prostate, Mr. Lawrence found the following useful. On the case he first makes these remarks:]

He generally passed his water twice a day, once before retiring to rest, when he often felt a desire to micturate more than he was able. His stream of urine had always been a full one, but he had experienced latterly some pain in the small of the back whilst micturating, and his

water had been rather turbid from a reddish sediment. No enlargement of the prostate could be felt by the rectum. What the incontinence depended on in this case was not very clear to me. Viewing it as being due to some loss of tonic contractility of the muscular fibres at the neck of the bladder, I prescribed the following mixture.

℞. Tinct. ferri sesquichloridi ʒii.; balsami copaibæ ʒi.; strychniæ gr. i.; infusi quassiæ ℥xii. M. Fiat mistura, cujus sumatur ʒi ter die.

To my surprise, the next time I saw the man he informed me that after taking two doses of the medicine the same day, he had no incontinence that night, and had not been troubled since. I certainly expected some effect from the remedies, but a cure so sudden and so permanent was as remarkable as it was gratifying.—*Assoc. Journal, Nov.* 10, 1854, *p.* 1017.

84.—APPARATUS FOR INJECTING THE BLADDER.

In various morbid conditions of the bladder the employment of injections, either acid, alkaline, or simply diluent, has long been a favourite practice with many surgeons. An ingenious expedient for the effecting of that procedure was recently brought under our notice by Mr. Wormald, who was then having recourse to it in a case under his care in St. Bartholomew's Hospital. It consists in the employment of a short piece of India-rubber tubing as a means of connexion between the syringe and the catheter. The appended wood-cut will best illustrate the mode. C represents the end of a catheter, B a portion of elastic tube, and A the nozzle of a common ear-syringe. The tube being a little smaller in bore than the size of either of the two instruments to which it is adapted, fits tightly, and requires nothing to secure it in place beyond its own elastic grasp. The adjustment of the three is perfectly easy: the tube having been fitted to the end of the catheter prior to the introduction of the latter, the conical nozzle of the syringe is afterwards readily admitted into its other end, and passed so far as to be firmly held. The advantages of the expedient are the following:—1st. The medium of connexion being flexible, all shaking of the catheter during the fitting of the syringe is prevented. This is of great importance, for it must be remembered, that in almost all conditions requiring the use of injections the mucous membrane of the

bladder is more or less inflamed, and can ill tolerate an additional irritation. When the whole instrument is metallic, it is almost impossible, during the fitting and removal of the syringe, to avoid more or less of joggling of the catheter. 2ndly. It obviates the necessity for a stopcock, since, as the tube is compressible, the finger and thumb may very conveniently serve all the objects of that appliance. 3rdly. It will connect any sized catheter with any sort of syringe. It is always expedient for the surgeon to be as far as possible independent of the instrument-maker, and suggestions which enable him to dispense with complexity of mechanical aids are very valuable. In injecting the bladder there are ordinarily employed a syringe of peculiar construction, and a stopcock with various nozzles of different sizes made to screw on, and graduated to fit catheters of different dimensions. By means, however, of a couple of inches of India-rubber tube all this complexity is obviated, and a common ear-syringe, or any other that may be at hand may, *extempore*, be adapted to a catheter of any size or sort. The plan under consideration has been subjected to many practical trials, both by Mr. Wormald himself and by the writer of this notice, and has been found to fully answer its intended objects. As an exhausting apparatus it is of course useless; but, by inserting the tube within one of a size larger, so as to make its sides sufficiently rigid, it may even be made to fulfil this purpose also. Exhausting syringes for the bladder are, however, but very rarely required.—*Med. Times and Gazette, Jan.* 6, 1855, *p.* 8.

85.—*Treatment of enlarged Testicle by Hutchinson's Apparatus.*—It is more than a year since the Clinique Allemande borrowed from the Wiener Medicin. Wochens. the description of Hutchinson's Air Compressor. This apparatus, which hitherto the English Journals (the Medical Times and Gazette excepted) have not thought fit to notice, consists of a double sac of impermeable stuff, folded on itself like a nightcap. An opening in front receives a short tube with a stop-cock, by which air can be admitted into the cavity. The testicle is put into the double sac, and the apparatus is fixed to the spermatic chord; the amount of air introduced between the two layers determines the degree of pressure. The organ is thus protected from direct violence; and any other medication may be simultaneously employed. Mr. Hutchinson considers pressure a most important agent in promoting absorption, and in controlling subacute and chronic inflammatory affections of the testicle. He thinks, also, that, with care, it may be the means of removing certain forms of hydrocele without the necessity of the use of the trocar.—*Annales Méd.*—*Med. Times and Gazette, Jan.* 27, 1855, *p.* 93.

DISEASES OF THE SKIN.

86.—GENERAL OBSERVATIONS ON CHRONIC DISEASES OF THE SKIN.

By Thomas Hunt, Esq., Surgeon to the Western Dispensary for Diseases of the Skin.

As these remarks will have a general bearing upon almost every kind of skin-disease, which is not specific in its origin, it will be convenient to introduce them in this place.

I. Local Treatment.—Perhaps no greater error in therapeutics is ever committed, than in supposing that cutaneous diseases of constitutional origin can be safely and permanently cured by topical applications. They may, indeed, by these means, be moderated, checked, relieved, and even dissipated; but, unless the application is absorbed, so as to effect a change in the circulating fluids, it is clear that, the source of the disease being untouched, it cannot be thus cured: and it is very doubtful if the cure can even be accelerated by local treatment. Relief may, however, be afforded by it, and sometimes to a great extent. Among these remedies may be enumerated:—

1. *The Tepid Bath.* This may be used daily, or less often. It acts first, by cleansing the skin, and removing extraneous sources of irritation; secondly, by cooling the surface, and thus arresting or assuaging inflammation; thirdly, by promoting insensible perspiration, which is often of general use to the system, as well as to the skin.

2. *Glycerine.* This fluid, when mingled with five or six times its its bulk of elder-flower water, is very useful where the skin is harsh and dry, or covered with hard crusts and scales, or disposed to crack. One application of this lotion will soften the skin, and moisten it for many hours; for the glycerine does not sensibly evaporate like other fluids, and the skin becomes permanently lubricated. It is said to exercise a specific and curative action in lepra; but of this I have seen no proof.

3. *Creasote*, in a very diluted form, will sometimes allay irritability, and act as a charm.

4. The *Solutions of Metallic Salts*, such as of the bichloride of mercury and the sulphates of zinc and copper, will occasionally be found useful for a time, when applied to the face, in acne or lichen, very much diluted with water.

5. The *German Poultice*, which consists of wet rags covered with oil-silk, is sometimes a source of great comfort in eczematous and scaly eruptions on the limbs.

6. *Ointments*, which generally do harm, are found useful in affec-

tions of the scalp, when the hair is not removed. They tend to separate the crusts. Those composed of calomel, white precipitate, and oxyde of zinc, or the nitrate of mercury, are the most useful. Tar ointment is in high repute; but it should only be used in hospital, dispensary, or parochial practice, and even then the remedy is often as annoying as the disease. These are all the applications I have found generally useful; but, as a general rule, it is better to apply nothing but what is necessary to cleanse the skin. Local treatment is apt to obscure the disease, or to drive it to other parts of the body, thus masking the progress of the improvement effected by the internal or general treatment, and leaving the practitioner ignorant of what is going on.

II. General Treatment.—The state of the general health is always the first thing to be inquired into. If that is obviously defective, it is useless to think about treating the cutaneous disease. About one-third of the patients suffering under chronic eruptions, complain that their health is not good. Constipation of the bowels, headache, and general dyspepsia, are very frequent complications. Suppression of the natural exhalation from the skin, of the urinary or uterine secretion, and of other natural or morbid discharges, are frequently associated with cutaneous disease. Occasionally all the secretions are more profuse than natural, and a state of general plethora exists. Sometimes an anæmic or strumous diathesis prevails. All these require appropriate treatment in the first instance; and, if this be successful in rectifying the general health, the skin commonly recovers its tone at the same time. If however, it fails to do so, the case may be treated specifically. The medicines generally useful may be described as *purgatives*, *sudorifics*, *diuretics*, *tonics*, and *alteratives*, which may be given alone or combined, according to circumstances.

Purgatives are the most generally useful. Almost every eruption is more or less benefited by their action; and there are many cutaneous affections which probably arise from the absorption of fæculent matter accumulated in the bowels. But, in order to derive the full benefit from purgatives, they should be administered for several weeks together; and the materials of which they are composed cannot be too numerous, or too frequently changed. The saline class are the most important; and, although they seldom suffice to cure the disease, they render the cure by alteratives much more easy and rapid. In these days of asthenic disease, when depletion is too generally condemned *in toto*, we are apt to lose sight of the invaluable advantage of clearing the bowels daily. If the diet is not stinted, debility seldom results; and it often happens that, by unloading the bowels, and thus enabling the function of defecation to be more easily performed, they act in effect as tonics, improving the appetite and spirits, and actually adding to the strength. When the patient is weak, great advantage is found in combining the sulphate of magnesia (for example) with the sulphate of iron, the disulphate of quina,

and dilute sulphuric acid. This often proves the best treatment of inveterate eczema in asthenic subjects.

Sudorifics are useful when the skin is dry and harsh, and the extremities cold. The warm bath, the liquor ammoniæ acetatis, and the patassio-tartrate of antimony are the most valuable. Dover's powder is sometimes useful in allaying extreme irritation.

Diuretics are not only useful when the blood is loaded with urates, but, when the urine is scanty in leucophlegmatic subjects, they are often essential to the cure. The acetate of potash and spirit of nitric æther in full does, with or without squills and colchicum, are very efficacious. If they fail, saline purgatives may be substituted.

Tonics are seldom required, except by the weak and ill-nourished class of patients. In dispensary practice, I have found the bebeerine an excellent substitute for quinine, in doses equal to those of the latter drug; and it can be obtained at about half the price. In emaciated or strumous subjects, Dr. de Jongh's light brown cod-liver oil is highly useful. This oil, though somewhat more expensive, goes three times as far as any other I have tried. One drachm is equal to half an ounce of the pale oil; and children will generally take it without objection. In some cases of lupus it acts as a specific. The iodide of iron is also useful in these case.

Alteratives are either primarily or ultimately essential to the successful treatment of a large majority of cutaneous cases. It is generally necessary in idiopathic cases to use some preparation of arsenic; for in most of them *all other alteratives fail.* The most manageable preparation is Dr. de Valengen's "solution of solvent mineral", now introduced into the Pharmacopœia under the cognomen "liquor arsenici chloridii." The dose is ten or twelve minims thrice a day, for all ages above two years. This dose is equivalent to five minims of Fowler's solution. This should be borne in mind, as its chemical composition has betrayed writers on pharmacy into the error of representing it as equal in strength to the liquor potassio-arsenitis. It should be given always immediately after a meal, and the dose slightly reduced if the eyes become irritated, or the eye-lids puffy. Patients will take it for many months without injury. If it fail, or disagree, the solution of arsenite of potass may be substituted for it; but it is far better that, in preparing this solution, the compound spirit of lavender should be left out. It is then tasteless and clear, and will keep, without becoming musty or decomposed, at least ten times as long. The dose is five minims for both adults and children. Children require, except in early infancy, as large a dose of arsenic as adults, and often larger. But some children are very susceptible of its toxical effects, and it is safer to begin with a smaller dose, which may be increased at the end of a week, and afterwards decreased, if necessary.

The signs of an over-dose of arsenic vary a good deal in different individuals; and yet they are comprised within a very few marked symptoms, the most common of which is a horizontal streak of red

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injection in the membrane lining the lower eyelid: this is soon followed by a slight itching or pricking in the eyelids, an increased secretion, and a puffy appearance in the lids and upper part of the cheek, as though the patient had been weeping. If the medicine is now pushed in full doses, or in increasing doses (as was formerly the practice), the tarsi become affected by pustular irritation at the roots of the eyelashes, which, if unheeded, may terminate, as I have once seen, in chronic induration of the tarsi.

The next most usual sign of an over-dose is irritation in the mucous membrane of the nostrils and fauces, with slight tickling cough, and a sense of fulness or stuffiness of the throat, simulating a common catarrh; but this symptom is so rare in summer, that I have seldom found it necessary to pay much regard to it. Still it is certain that arsenic will aggravate a severe catarrh.

Irritation of the bowels I have found to be a very rare result of arsenical treatment, provided the medicine is not given on an empty stomach. Still more rare is irritation of the urethra; but I have seen, in two cases only, a slight gleet, and in one a purulent gonorrhœa, produced by arsenic, in each case recovering as soon as the medicine was withheld. Now and then, tenderness, and vesication of the soles of the feet, occur, but not until after a lengthened course of arsenic; and, in fair individuals, a very delicate desquamation of the skin in the parts covered by the clothes, giving a dirt-brown appearance, is by no means uncommon. This gradually disappears on the termination of the course, leaving the skin more delicate, fair, and smooth, than before its commencement. Some individuals (about one in two hundred) are unusually susceptible of the toxical effects of arsenic. The face swells; the voice is husky and thick; the breathing is short; a sense of sickness, oppression, and fulness, at the precordia, is felt; and the nervous system is prostrated. These symptoms I have once seen produced by half a drachm of Fowler's solution, in divided doses. Some of these patients will bear one minim for a dose very well, and the skin will recover under it; but it is more prudent, in other cases, to abandon the medicine altogether. In some patients, arsenic affects the nervous system at first, producing general depression, tremor, and insomnia; but these symptoms are often only temporary, whereas the vascular disturbance is apt to increase by perseverance with the remedy. In others, the appetite fails; but this seldom continues long.

I have now exhibited arsenic in several thousands of cases, and and have kept a minute record of almost every case; and the above are the only toxical symptoms I have noticed which could be reasonably attributed to the medicine in the cautious method of administration above described. I have generally found it perfectly manageable; and, when prescribed under favourable indications, generally useful. In very few cases have I been obliged to abandon it.—*Assoc. Journal, Jan.* 5, 1855, *p.* 13.

87.—USE OF AN EXTRACT AND DECOCTION OF THE COMMON STINGING NETTLE IN SOME CHRONIC SKIN DISEASES.

By Dr. J. Bullar, Physician to the Royal South Hants Infirmary.

[The following cases of Dr. Bullar's are interesting. The medicine seems to have been chiefly useful when the general system of the patient was *cachectic.*]

Case 1.—A young lady, aged 14, of strumous habit, had been liable for some years before I saw her, to occasional eruptions of psoriasis diffusa in large patches over the flexures of the knee and elbow-joints, attended with considerable redness, irritation, and heat. Twice (in Dec. 1849, and June 1850,) it was removed by liquor arsenicalis prescribed by myself. The arsenic gave her headache; so that she was averse to taking it. The eruption returned a third time after an interval of three or four months (Nov. 1850); patches of psoriasis also appeared on her forehead, neck, and chest, and resisted the liquor arsenicalis although given for several weeks. I then ordered a quarter of a pint daily of the decoction of the urtica dioica, made by boiling an ounce of the leaves and stems in a quart of water until it became a pint. She rapidly improved, and after taking five pints was completely cured, and has had no return. The only effect she perceived was an improvement of her appetite.

Case 2.—Mary C., aged 30, pale and cachectic, with habitually costive bowels, had a dry, chronic, scaly eruption surrounding the ankle-joint and lower part of the right leg, of three years' standing. The same disease had affected the whole leg; the upper part had been cured, but the lower remained stationary. and for the last twelve months she had been subjected to no treatment. It was like the remains of chronic eczema. She was ordered to take a quarter of a pint of the decoction of nettle, and she returned in a month quite cured, having felt no other effect from the medicine.

Case 3.—Edward H., a delicate boy, aged 4, had an inveterate eruption of lichen agrius covering nearly the whole of the back, chest, and abdomen, of two years' standing. The edges alone showed its papular nature, the rest, from inflammation and scratching, was covered either with dry scabs, or with thin scales, thickening and hardening the skin itself, I recommended that he should take half a pint of the decoction of nettle as his common drink, and he soon liked it. To my surprise, in five or six weeks the whole eruption was removed, and his skin was clean, smooth and natural, and not even marked. In this case, the cure of the skin disease seemed to have been too rapid, as he was soon brought to me again complaining of pain in his loins, and looking pale and ill: his bowels were costive. These symptoms were relieved by mild aperients and alkalies. Subsequently there was a slight return of the eruption, requiring a return

to the decoction. During its use he sometimes complained of pain in making water.

The rapidity of the amendment in this case of a chronic eruption, affecting almost the whole surface of the trunk, in a cachectic child, which had lasted two years, and was completely removed in five or six weeks, gave me a strong impression of the power of the remedy.

Case 4.—Edward W., aged 8, a delicate strumous child, of an unhealthy mother, had had for six months a patch of chronic eczema covering both eyelids, the eyebrow, and part of the cheek of the left side. It was covered with a dry scab. He was out of health, languid, and complained of headache. Constitutional treatment, with calomel ointment and subsequently pitch ointment, were of little service. He was then ordered a quarter of a pint of the decoction of fresh nettles daily. In a month, the eczema was well. He was much better in health, having lost the languor and headache. The ointment of nitric oxide of mercury was used locally.

The preceding cases are selected to prove the beneficial action of the decoction of the nettle; the following cases show that the extract has similar virtues.

Case 5.—J. S., aged 27, a strong man, of fair complexion and light hair, and of healthy appearance, had patches of chronic lepra covering the elbows and knees, and extending above and below them for a considerable distance. The diseased service was covered with thick scales like white mortar, entirely obscuring the red surface beneath them. The eruption had existed twenty-four years. His general health was excellent. He had never taken medicine to remove it; but as he was now confined to his bed in the South Hants Infirmary with a broken leg, he was willing to have the disease removed, and was a very fit case to try the remedy. He commenced with five grains of the extract three times a day. After three or four days there was slight improvemenc; after sixteen days, when he had taken half an ounce of the extract, the improvement was striking, as the thick white scales covering the affected parts were removed, and the parts beneath were almost as smooth as natural. During the prevalence of dry easterly winds the disease was stationary, and the dose of extract was increased to twenty grains daily; and five weeks after he had commenced it, he left the infirmary quite well, with only the marks on the skin where the eruption had been. He was conscious of no perceptible action of the medicine; but his breath had a smell of herbs.

Case 6.—W. L., aged 22, a young man with light hair, fair skin, blue eyes, and otherwise healthy aspect, had been covered with lepra vulgaris for nine months. His body, legs, and arms, were closely spotted over with circular patches; and the forehead and hairy scalp were covered with a more diffused scaly eruption. He had been three months under the care of a physician in a neighbouring town, of much practical skill, who had fairly tried the liquor arsenicalis, but without

any improvement. After taking five grains of the extract of urtica dioica three times a day for a week, the eruption itched more, but the scales were less numerous; and, on persevering for a month, the forehead and scalp were quite well, and the eruption on the body was fast dying away; the scales diminishing, and the surface beneath becoming paler. At the end of six weeks he reported himself as quite well, and I found the marks alone remaining.

Case 7.—M. N. aged 40, a stout woman, who was nursing a child three months old, was admitted into the South Hants Infirmary, with inveterate eczema impetiginodes of twelve years' standing. The eruption completely covered both hands and the forearms; and there were large patches on the arms, shoulders, chest, neck, and face, disfiguring her, and much interfering with her work, as her hands were chapped and sore. The itching was very troublesome. Her general health was good. She had been under medical treatment for many months by an experienced surgeon in the neighbourhood, who at last, as she did not improve, sent her into the infirmary. I ordered a warm bath and five grains of the extractum urticæ dioicæ to be taken three times a day. After taking this for a few days, she complained of a stinging feeling in her skin, which she had not had before. The eruption slowly but decidedly diminished. Her hands and arms, which had been covered with an agglomeration of pustules, vesicles, scabs, dry scales, excoriations, and deep chaps, were in about six weeks quite well. She was discharged eight weeks after admission, with merely the remains of vesicles on the shoulders and neck, and with directions to take the decoction of the fresh nettle at home. She returned, two months afterwards, perfectly well. During the course of her treatment in the infirmary, pitch ointment was applied to the eruption on one of the shoulders, to relieve the distressing itching, and with some advantage.

Case 8.—J. F., aged 8, a brunette, of a somewhat strumous organisation, but not unhealthy looking, though delicate, was affected with a dry, furfuraceous, hardened, and rather red condition of the palms and palmar surface of the fingers and thumb of both hands. They were chapped, and she complained that they were painful and itched. The complaint was of four months' standing. She was ordered to take five grains of the extractum urticæ dioicæ three times a day. In five weeks (after taking half an ounce of the extract), her hands were well. They were free from pain and itching; the colour was natural; and there was no dry scaling epidermis; but the skin was not so soft as it should be, though no harder or drier than is natural to some hands. The general health was improved.

Case. 9.—Ellen C., 11 years old, and of scrofulous habit, had been subject for eight years to unhealthy strumous sores on her hands. They commenced either as vesications or as flat livid tubercles, about the size of sixpence, and as much elevated above the skin; superficial ulcers formed, which healed with great difficulty, leaving white scars.

Various means had been tried without success; and, as the complaint seemed to depend on a cachectic state of the system, I ordered, on April 5th, 1851, five grains of the extract of urtica twice a day. She rapidly improved; the sores healed, and the tubercles diminished in a fortnight. On leaving off the medicine, a few fresh vesicles formed; but, on resuming it, these healed, and in a month her hands were well. At the end of three months there had been no return.

Case 10.—Elizabeth C., aged 6, a fine, light-haired child, whose general health was usually good, but who was strumous looking, had been affected for some weeks by a dry papular eruption of the back, chest, arms, and legs, with some distinct pustules on the hands and arms. The papular eruption was lichen agrius; but the pustules had so much the appearance of pustular itch, and were attended by so much itching that I ordered (October 18th), sulphur ointment to be freely applied, and diluted sulphuric acid with quinine twice a day. This was continued for twelve days, when the pustular eruption was quite well, but the lichen was not improved. She was then ordered to take five grains of the extractum urticæ dioicæ twice a day.

Nov. 8th. She had taken two drachms, and the eruption was much diminished.

Nov. 28th.—She had taken three drachms more, and the eruption was almost well. After this she took three drachms more; and her skin was then free from any eruption, and her general health excellent.

Case 11.—S. P., aged 15, on October 28th, 1852, had large patches of lepra vulgaris on the legs, and smaller ones on the thighs, of two years' standing, and gradually getting worse. Menstruation was scanty; she complained of giddiness and aching in her limbs. I ordered her to take five grains of extract of urtica three times a day.

Nov. 10th. She had taken four drachms, and the eruption was declining. She felt better, and had less pain in her limbs.

Dec. 21st. She was very much better. She had taken one ounce. When giddy, I ordered her to take an aperient.

Jan. 27th. She was quite well, having taken another ounce. As she had not menstruated, she was ordered to take iron.

Case 12.—Mrs. W., a fat florid woman, about 50, had had for six years lichen agrius, chiefly over the extensors of both arms, and of the skin covering the chest, back, and abdomen. She had been under treatment by various practitioners without benefit. At one time, she was ten months continuously under one, and at another period six months under another, until she "was tired of taking medicine." The itching was considerable. In addition to getting the secretions right by alterative mercurials and aperients, she was directed to take a quarter of a pint daily of the decoction of nettle, and subsequently five grains of the extract twice a day. She began on May 18th, and on the 29th of August the eruption, which had gradually declined,

was quite well, and remained so when I saw her five weeks afterwards.

Case 13.—Mrs. A., aged 43, July 9th, 1854, for six months had had lichen agrius of both arms. Both legs were affected with a diffused deep red eruption, with much itching, but without discharge. Sulphur ointment was applied to the legs; five grains of compound rhubarb pill were taken every night; and five grains of the extract of urtica twice a day.

July 31st. She had taken one ounce of the extract, and was much better.

Aug. 7th. The legs were nearly well, but there was rather more eruption of lichen. The extract was omitted.

Aug. 21st. The lichen was in the same state. She was ordered to resume the urtica.

Aug. 28th. The lichen was well, and the legs also.

In this case, as not unfrequently happens, the urtica seemed occasionally to aggravate the eruption; and, in the following, it seemed to bring it out, tending to show its real efficacy on the system.

Case 14.—A. T., aged 44, a tailor, had inveterate psoriasis palmaria, of four months' standing; both palms were covered with dry hard epidermis, and tender from numerous chaps. He was directed to apply glycerine, and to take a quarter of a pint of the decoction of urtica daily. After taking the decoction for a week, he became almost covered with a papular eruption of lichen, which itched considerably. His hands felt better. On omitting the nettle, the papular eruption declined and desquamated. He resumed it, and, with the constant use of glycerine, his hands, when he last called on me, were almost well, and quite comfortable.

These are a few out of a much larger number of cases in which I have tried the urtica during the last two or three years, as extract and decoction. From this experience, it seems to be particularly indicated in inveterate chronic papular eruptions (lichen), and in various forms of vesicular and scaly diseases (eczema, lepra, and psoriasis), especially when occurring in cachectic states of the system. By those engaged in the country as parochial surgeons, who have numerous cases of chronic skin diseases in unhealthy children, and who have not already employed this remedy, it is worth a trial, from the facility with which it can be procured by the poor.

It is hardly necessary to add that, if the tongue is loaded, it is advisable to commence with mild mercurials and aperients, before the urtica is used; and that, in all cases, a plain wholesome diet, and frequent washing of the whole skin, are to be recommended. Indeed, to prevent the return of skin diseases which have been of long standing, it is of primary importance that the whole surface of the body should be washed daily with soap and water; for, as eruptions are often the means which nature sets up for relieving a faulty state of the system through the skin, it is rational to suppose that a free and

perspirable condition of that extensive surface will assist nature in removing her waste matter, and thus will prevent the necessity of this scaly, or papular, or vesicular action. What the older practitioners attempted by means of issues may be more naturally effected by the daily use of soap and water to the whole cutaneous surface. In one case, this practice alone, steadily persevered in for several months, and continued as a habit, permanently cured psoriasis palmaria of more than twenty years' duration.

Washing the whole surface daily with soap and water will be found to be a very serviceable recommendation, not as a mere matter of cleanliness, but as one of the directions for the general treatment of of many chronic diseases, and for the preservation of health in advancing life. Its effect is very different to that of merely washing the body with cold water, or using shower baths, or bathing followed by dry friction. It does not give the same shock as cold water alone; and it is thus much more easily borne by the delicate, who find that the reaction after cold sponging is exhausting. It more effectually removes the waste epidermis than flesh-brushes or hair-gloves; and suits those whose delicate skins are often irritated into eruptions by these rough appliances. For those who are very subject to colds, and are yet too delicate to bear cold sponging, the addition of soap, by its stimulating property, acts as a tonic to the skin, and thus guards it against the effects of sudden changes of temperature, besides aiding in getting rid of the waste and superfluous matters, which often require the actions which constitute a cold for their discharge from the overloaded system. The yellow soap, from the resin it contains, is an excellent stimulant as well as detergent; and a better still, in many cases, is a soap containing a small quantity of Barbadoes tar, called Hendrie's petroline soap, the daily use of which is peculiarly serviceable in keeping up a smooth and perspirable state of the skin when it is naturally dry, and especially in declining life, when the loss of muscular power prevents exercise carried to perspiration; for, as man is organized to earn his bread by the sweat of his brow, if he cannot do this, he must use some artificial means to produce the same state of his perspiring organ, or he will suffer. A physician, distinguished in his day in a neighbouring county, who now, though verging on 90, retains a freshness and vigour of intellect, as well as a sound condition of health, which enable him to enjoy his existence physically and socially, and to devote a portion of each day to the cultivation of science, informed me that he attributed much of his health and activity to this daily soaping of his skin, which he had persevered in since he was a very young man. And the elderly, who usually have much spare time at their disposal, will find that the employment of a portion of it daily in this way will repay them for the pains. In the management of the disorders of the general health which so often attend the cessation of menstruation, this practice is, for obvious reasons, to be recommended.—*Association Med. Journal*, *Nov.* 10, 1854, *p.* 1910.

88.—ON DISEASES OF THE SKIN.

By M. Bourguignon.

[M. Bourguignon has communicated to the Society of Medicine of Paris, a report on M. Devergie's "Practical Treatise on Diseases of the Skin," from which the following extract is taken. M. Devergie divides skin diseases into twelve groups, viz.:]

1st, exanthematous affections; 2nd, vesiculæ and bullæ; 3rd, pustular diseases; 4th, cachetic; 5th, papular; 6th, scaly; 7th, exotic; 8th, scrofulous; 9th, those of the scalp; 10th, those of the nails; 11th, accidental productions; and 12th, syphilitic.

This classification is in truth as good as any other, although it makes it necessary to treat many times of the same disease; thus, for example, eczema, impetigo, ecthyma, studied amongst the groups of vesicular and pustular diseases, are a second time found among the affections of the scalp; this, however, we repeat, is, a detail of minor importance.

It does not displease us to see the itch driven from the group of vesicular diseases to take its place amongst the papular. At last, no doubt, it will be ranged amongst the number of parasitic diseases.

M. Devergie excels in describing the various pathological forms that the anatomical elements of the skin assume. We would mention, among others, as having particularly attracted our attention, his description, amongst the exanthemata, of pellagra; amongst the vesicular diseases, of eczema, which sometimes assumes a hybrid form; of herpes zoster; and of chronic pemphigus, a complaint that never quits those who are unfortunate enough to be attacked by it.

The author has treated with particular care of rupia among the cachectic affections, and of the itch among the papular; he exposes, at great length, the reasons which induce him to believe in the *spontaneous generation* of acari; he believes, in short, the presence of the insect to be the result of disease, and that we have, up to the present day, mistaken the cause for the effect.

This pathogenic doctrine is, in our eyes, of very great importance; it is my intention to treat of this subject more fully afterwards.

In the sixth group we shall mention the description of psoriasis; where there will be found a notice of the different modes of treatment, compared with that by means of arsenic; in the eighth group we find the scrofulous diseases which the author attributes to a degenerated and transformed syphilitic taint, and which, although somewhat removed from the special field of dermatology, will be read with veritable interest. Among the affections of the ninth group I will quote tinea or favus, which has been abandoned almost exclusively to the medication of empirics, inasmuch as, in July, 1853, a peasant, who devoted himself, in his native village, to the cure of this disease, showed to M. Bazin the means of treating it by depilation. The twelfth group contains the chapter on syphilitic diseases, in which the author expresses his opinion on a subject much disputed, viz., regarding the character-

istic form of the primary, and the contagiousness of the secondary accidents.

Such is a brief sketch of the descriptive part of the work. I would gladly dwell longer upon it, but I prefer fixing your attention on the chapter which treats of the general pathology, which seems to me to be the most excellent part of the "Practical Treatise on Diseases of the Skin."

M. Devergie expresses his astonishment at the isolated position which the diseases of the skin still hold in reference to affections of other organs.

He observes, that the diseases commonly known under the name of herpetic affections are, in every respect, identical with similar inflammations of other tissues. He attributes their origin to the same cause, and assigns to them the same progress and termination; he does not seek for an explanation of the disease in any morbid element peculiar to itself, nor in any specific virus, but in the temperament of the subject, his constitution, hereditary tendency, climate, moral causes, privations, and former diseases; in a word, M. Devergie conceives that the pathology of the skin does not differ from the pathology of other tissues and organs, except in the varied form of its morbid productions. Accordingly to him, the morbid elements and causes are identical, and consequently, there ought to be a close similarity in the therapeutics of these and other diseases.

In order to prove that his doctrines are well founded, he lays down the following general principles:—

1st. That the generality of diseases of the skin have a certain morbid inflammatory character, such as is seen in the greater number of the diseases affecting the various tissues of other organs in the economy.

2nd. That the diseases of the skin may be produced by all the causes which give rise to diseases of other tissues or organs, as physical, moral, and hereditary causes, climate, temperament, constitution, &c.

3rd. That the diseases of the skin follow in their progress the same course, and present the same termination, as the diseases of other organs.

4th. That if the morbid forms met with in cutaneous diseases are very varied, it is because the tissue of the skin is the most complex of all the tissues in the economy.

Then there follows the enumeration of some diseases due to each of these orders of causes.

The treatment is dwelt upon with particular care, and is inferior to nothing which has ever been written on the subject of therapeutics. In the treatment of cutaneous affections the author wishes to keep in view the disease itself as well as the phenomena that it developes, their cause, and, above all, the temperament of the patient; for certain morbid forms are always connected with certain temperaments, and each temperament demands a peculiar plan of treatment.

On the subject of therapeutics he successively treats of the following:—

A. Of the antiphlogistic treatment.
B. Of the antilymphatic treatment.
C. Of the antipapular treatment.
D. Of the antisquamous treatment.
E. Of the mixed or compound treatment.

These considerations on the subject of therapeutics are concluded by an excellent article on baths in general.

The antiphlogistic treatment comprises bleeding, emollients, and purgatives, from which it is necessary to abstain in the treatment of rupia and pemphigus; resolvents, in solution, as acetate of lead, nitrate of silver, alum and corrosive sublimate; and in pommades, as oxides of zinc, tannin, camphor, chloroform, &c.

The antilymphatic treatment comprises bitter preparations, and such as are of an antiscorbutic nature, as the chlorides of barium, of gold. or of calcium, preparations of iron, and cod liver oil.

The antipapular, or antinervous treatment, comprises alkaline preparations in baths, drinks, and pommades; the doses of the subcarbonate of soda and of potash are remarkably diminished in comparison with the formulæ generally adopted. Amongst the alkaline waters those of Louèches, Vichy, Neris, Bourbon-Lancy, deserve a preference, especially in reference to chronic lichen. This mode of treatment also comprises the local use of chloroform, camphor, &c., so efficacious in complaints attended with much itching, and also tincture of cantharides, given in doses of one drop night and morning, gradually increased to ten drops.

The antisquamous treatment consists of the arsenical preparations so useful against psoriasis. M. Devergie lays much weight upon their administration, not only in this variety of diseases of the skin, but in a number of other affections, equally chronic and rebellious to treatment. In theory, and according to the idea that we have ourselves formed of the primary causes which remotely produce diseases of the skin, the use of arsenic seems to us to be valuable in exactly the same way that the preparations of iodine and mercury are of use against the syphilitic affections of the skin. Certain cases in which the rebellious eruptions of the skin alternate by metastasis or by repercussion with diseases of the stomach and of the womb, have very much arrested our attention, whilst the curious articles published by all the press concerning the arsenic-eaters have caused us to make some reflections on the administration of such preparations. In fact it is with the most lively interest that we treat of the therapeutical and physiological effects of arsenic. And if it be true, as the assertions of Dr. Tschudi do not permit us to doubt, that the inhabitants of Southern Austria, Styria, and the Tyrol, often make use of arsenic in comparatively large doses, in order to produce an appearance of freshness and *embonpoint,* and to facilitate the functions of respira-

tion; and if it is equally incontestible that the breeders of horses and oxen give it to these animals in order to excite in them the power of getting fat, and to give fineness to their coats, we can better understand the good effects of arsenical preparations in the scaly affections. Arsenic will, no doubt, after these recent publications, have its period of fashion, but assuredly it will always remain as a valuable remedy against a great number of cutaneous diseases hitherto considered incurable. M. Devergie refers to the action of arsenious acid, which has been found much less beneficial than that of the arsenites and arseniates; and it is important to bear this in mind in the administration of so poisonous an agent.

He was the first to observe a brown discoloration of the skin, named *arsenical spots*, when this drug is pushed to saturation.

The mixed or compound treatment is chiefly recommended in syphilitic affections; it principally consists in a solution of two and a half drachms of iodide of potassium in fifteen ounces of water, of which a teaspoonful is to be taken night and morning in a cup of sudorific infusion. An hour after, M. Devergie administers corrosive sublimate in the form of a pill, with extract of opium.

He sometimes orders a syrup, containing—

Iodide of potassium, ℥ijss.; corrosive sublimate, gr. i. to iss.; thebaic extract, gr. iij.; syrup, ℥xv. M.

This mixture he prefers to the association of iodide of mercury with iodide of potassium, so as to form hydriodate of mercury, for in this case it is an error to suppose that any notable dose of iodine is administered.

In the scrofulous syphilis he gives the cod-liver oil, the syrup of iodide of mercury, and afterwards iron. For tertiary symptoms, he adds an arsenical preparation, Fowler's solution, in doses of from one to ten drops. In those skin diseases which are essentially scrofulous, he recommends cod-liver oil, wine of gentian, syrup of iodide of iron, all mixed together. In lupus, he gives cod-liver oil in doses of eight to ten teaspoonfuls every day.

I wish to draw attention to this plan of associating together these medicaments, which are in general administered separately, because this fact shows the peculiar tendency of our author's views; we know that he believes in the compound forms of diseases of the skin; he continues faithful to this principle in their treatment.

A chapter upon baths, and that by no means the least instructive of the work, completes the part devoted to treatment. Baths act in various ways, owing to their composition, their temperature, and the length of time the patient is subject to their influence; and in the douche bath by the violence of the jet; thus the douche with a single jet is best suited for neuralgia, that with many jets for paralysis.

The vapour baths are made the subject of a minute and interesting description; they act by the production of pulmonary and cutaneous perspiration.

They ought to be, in diseases attended with secretion, of a temperature of about 35 degrees *cent.* when the patient enters; afterwards they should be raised to 38, 40, or 42 degrees. In rheumatism the temperature ought, on the contrary, to be carried from 50 to 55 degrees, because in this case it is necessary to excite the skin more powerfully. A vapour bath should not last more than from 20 to 25 minutes; when it causes palpitations, it is because the temperature is too high. In certain cases, two or three shower baths of comparatively cold water may be given with excellent effect, especially in the cutaneous affections of the face. When a Russian bath is administered, the vapour is raised to 55 degrees, and cold douches are administered every five minutes.

The vapour baths do not disturb digestion; it is preferable that they should not be given when fasting.—*Rev. Med.—Dub. Hospital Gazette, Nov.* 1, 1854, *p.* 296.

89.—ON THE TREATMENT OF THE MORE COMMON FORMS OF SKIN DISEASES.

By Dr. J. H. Bennett, Professor of Medicine in the University of Edinburgh.

Eczema is by far the most common disease met with, both in its acute and chronic forms. The local treatment I have found most efficacious is that which I recommended, in the August number of the Journal for 1849. It consists in keeping the affected part moist, with lint or linen saturated in a very weak alkaline solution, consisting of soda subcarb. ʒss. to a pint of water. For this purpose it is necessary to cover the moistened lint with oil silk, or gutta percha sheeting, which should well overlap the lint below, so as to prevent evaporation. The usual effect is soon to remove all local irritation, and especially the itching or smarting so distressing to the patient; to keep the surface clean, and prevent the accumulation of those scabs and crusts, which in themselves often tend to keep up the disease. After a time, even the indurated parts begin to soften, the margins of the eruption lose their fiery red colour, and merge into that of the healthy skin, and, finally, the whole surface assumes its normal character.

Herpes.—This disease generally runs its course in about fourteen days, and requires no treatment whatever, further than an acetate of lead lotion to allay the smarting. It is not very common.

Scabies occurs very frequently, and is cured by a host of remedies. A strong lather, made of common soft soap and warm water, twice a day, answers very well. The question with scabies, is not what remedy is useful, but which will cure it in the shortest period. The most extensive experience at St. Louis has shown, that the sulphur and alkaline, or Helmerinch's ointment cures itch, on an average, in

seven days. That sulphur, however, is not the active remedy, I have satisfied myself of by experiment. Soft soap, as we have seen, which contains alkali, and even simple lard, if pains be taken to keep the parts constantly covered with it, will cure the disease as soon as sulphur ointment. I have tried the Staveacre ointment, recommended by M. Bourguignon, in only a few cases, but found it to answer very well. Its superiority, however, over other applications, I am not as yet prepared to admit.

Pemphigus.—This is rather a rare disease, and when chronic, coming out in successive crops, is very rebellious. Two cases which entered the infirmary last winter were cured in a few weeks, by the weak alkaline wash, applied as in the case of eczema, combined with generous diet.

Impetigo.—This affection in all its forms is very common, and is best treated by the weak alkaline wash, exactly the same as in eczema. In the chronic forms which attack the chin of men, constituting one of the varieties of mentagra, the same treatment cures the most rebellious cases, if the moisture be constantly preserved. For this purpose the hair must be cautiously cut short with sharp scissors, and the razor carefully avoided. If the side of the cheek covered by the whisker be attacked, removal of the hair from thence also is essential to the treatment. A bag or covering accurately adapted to the part affected must be made of gutta percha sheeting, and tied on with strings. This may be covered with a piece of black silk, to allow the individual to go about and carry on his usual occupations. In this way I have frequently seen chronic impetigo of the chin, of from eight to ten years' standing, completely removed in a few weeks. But then the surface must be kept *constantly* moist, a circumstance requiring very great care and determination on the part of the patient. When it becomes necessary to shave, flour and warm water, or paste, should be used, and not soap. Alkalies applied from time to time only, as in the form of wash or soap, always irritate, although, when employed continuously, they are soothing.

Ecthyma is not a common disease, and usually presents itself, as the *E. cacheticum*, requiring in addition to the alkaline wash locally, a generous diet.

Acne is a disease always requiring constitutional rather than local remedies. Although not uncommon in private, it is rare in hospital practice. Careful regulation of the diet, abstinence from wine and stimulating articles of food, watering places, baths, &c., &c., constitute the appropriate treatment.

Rupia.—This disease I have never seen occur but in individuals who have been subjected to the influence of mercurial poisoning. Hydriodate of potassium and tonic remedies, with careful avoidance of mercury in all its forms, is the treatment I have found most successful.

Lichen and Prurigo.—In both these affections constant inunction with lard is as beneficial as constant moisture in the eczematous and impetiginous disorders. In the prurigo of aged persons, the Ung. Hyd. Precip. Alb. is a useful application, although the disease is not unfrequently so rebellious as only to admit of palliation. The chronic papular diseases often constitute the despair of the physician.

Psoriasis, and that modification of it known as *lepra*, are very common diseases, and are uniformly treated by me externally with pitch ointment. I have satisfied myself by careful trials that it is the pitch applied to the part that is the beneficial agent, as I have given pitch pills, and infusion of pitch, largely internally without benefit. With the hope of obtaining a less disagreeable remedy, I have frequently tried creosote, and naptha ointment and washes, but also without benefit. Lastly, I have caused simple lard to be rubbed in for a lengthened time, but without doing the slightest good. The oil of cade is also very useful, especially in psoriasis of the scalp. Internally, I give five drops each of Fowler's solution, and of the tr. cantharidis. It is rare that the internal treatment alone produces any effect on a case of psoriasis of any standing. If a case resists this conjoined external and internal treatment, I have always found it incurable. About a year ago I carefully treated a series of cases internally, with Donovan's solution, without producing the slightest benefit.

Lupus is a constitutional dicease, and must be treated by cod-liver oil, and all those remedies useful in scrofula, of which it is a local manifestation. The external treatment is surgical, consisting of the occasional application of caustics, redlotion, ointments, &c., according to the appearance of the sore.

Favus is a very common disease in Edinburgh, and is most readily removed, first, by poulticing the crusts till they fall off, and the skin presents a smooth, clean surface; secondly, by shaving the hair; and, thirdly, by keeping the scalp continually covered with oil, so as to exclude the atmosphere, and prevent the growth of the parasitic fungi, which constitutes the disease. For this purpose, a gutta percha or oil silk cap, must be constantly worn. A continuance of this treatment for six weeks produces a cure in young persons, if combined with cod-liver oil, generous diet, and anti-scrofulous remedies internally. I have tried the lotion of sulphurous acid, recommended by Dr. Jenner, and found it successful in a few cases, but the treatment by oil is so easy as to be far preferable to it. Very chronic cases are cured with difficulty, but so long as the oil is applied the disease never returns, and mere freedom from the disgusting crusts is a great gain.

Scalp diseases must be treated according as it depends on eczema, impetigo, psoriasis, or favus, in all cases first removing the crusts

with poultices, then keeping the head shaved, and, lastly, applying alkaline washes, pitch ointment, or oil, according to the directions formerly given. Ringworm is a disease I have never seen in Edinburgh, and of what it consists I am ignorant. Some writers apparently consider it to be favus, and others a form of herpes. On two or three occasions I have seen a scaly disease of the scalp, in the form of a ring—that is lepra, which I have cured by pitch ointment, or oil of cade. My friend, Dr. Andrew Wood, informed me some time ago, that he banished it from the Heriot's Hospital school by condensing on the eruption the fumes of coarse brown paper, and thus causing an empyreumatic oil, or kind of tar to fall upon the part. This has led me to suppose that it is a scaly disease, and a form of lepra or psoriasis.

So-called *syphilitic diseases* of the skin, are in my opinion, the various disorders already alluded to, modified by occurring in individuals who have suffered for periods more or less long, from the poisonous action of mercury. A longer time will be required for their cure, but the same remedies locally, conjoined with hydriodate of potassium, in smaller doses, with bitter infusions, tonics, and a regulated diet, offer the best chance of success.

The great difficulty in the treatment of skin diseases, generally consists in their having been mismanaged in the early stages—a circumstance I attribute to their not having, until a recent period, been much studied by clinical students. Many chronic cases of eczema are continually coming under my notice, which, in their acute forms, have been treated by citrine ointment, or other irritating applications, which invariably exasperate the disorder. I shall not easily forget the case of one gentleman, covered all over with acute eczema, who had suffered excessive torture from its having been mistaken for psoriasis, and rubbed for some time with pitch ointment. In the same way I have seen a simple herpes, which would have readily got well if left to itself, converted into an ulcerative sore, by the use of mercurial ointment. Nothing is more common than to confound chronic eczema of the scalp with favus, although the microscope furnishes us with the most exact means of diagnosis. I need scarcely say that the correct application of the remedies I have spoken of can only be secured by an accurate discrimination, in the first instance, of the diseases to which they are applicable.

The general constitutional treatment in all these cases seldom demands aperient or lowering remedies except in young and robust individuals with febrile symptoms. In the great majority of cases, cod-liver oil, good diet, and tonics are required. In a few instances, sedatives, both locally and internally, are necessary to overcome excessive itching and irritation. These the judicious practitioner will readily understand how to apply according to circumstances.—*Monthly Journal of Medicine, Jan.* 1855, *p.* 19.

90.—ON PLASTIC SURGERY.

By T. Spencer Wells, Esq., Surgeon to the Samaritan Hospital.

When a part covered by cicatrix has to be restored, the cicatrix should be entirely removed. Suppose, for example, the point of the nose to have been destroyed; it is not sufficient to pare the edges of the defect and annex the flap to the denuded edges; it is absolutely necessary to remove very carefully the whole cicatrised surface of the nasal stump which is to be covered by the engrafted flap. Of course this denudation of cicatrix is to be effected with the greatest forbearance towards any remaining sound portions of the defective part. Langenbeck has insisted very strongly upon these points, and with great reason, especially in cases where the free borders of the facial integument, the lips, the edges of the eyelids, and the alæ nasi are concerned; for, as these parts can never be restored in their original perfection and delicacy, any portions of them which remain must be most carefully preserved. In two cases, where the whole of the cartilaginous portion of the nose was destroyed up to the very narrow remains of the free edges of the alæ nasi, he pared these edges, and united them by means of fine sutures with the flap transplanted from the forehead, so that the natural contour of the nostrils was most perfectly restored. In other cases he has preserved the tarsal border of an eyelid otherwise destroyed, and to this annexed the flap, so that the new eyelid, after healing, has been furnished with eyelashes along its whole border. I have followed this proceeding in one case with complete success.

If a transplanted flap were simply united with the denuded edges of a defective part, and the raw surface of the flap were applied upon the old surface of cicatrix covered by a more or less normal epidermis, the two surfaces would never unite, the flap would become raised, thickened, and undergo considerable alteration in form and appearance. The whole of the epidermoid surface of the cicatrix should therefore be pared away, so that two raw surfaces may be brought into apposition.

In order to bring the edges of the flap and the pared edges of the defect into perfect apposition, the latter must be loosened from their attachments to the subjacent parts, to the extent of two, three, or four lines, by flat strokes of the knife. This is too often neglected, and the consequence is, that it is found difficult to bring both edges to the same level. One projects beyond the other, and the flap remains raised even after union by first intention. But if the edges of the defect be so loosened that they can be raised from their attachment, both the edges are easily brought into close apposition, and no difference of level is observed after union takes place.

As very small defects can always be restored by some of the simpler methods, no very small flap can ever be required, certainly not less than half an inch in breadth. The largest flaps ever employed have been about six inches by five, and have been removed from the shoulder to fill up gaps left in the neck after the division of cicatrices.

The size of the nourishing peduncle varies very much with the nature of the operation. In forming a nose from the arm, it is left as broad as the flap itself. It is also left very broad when large flaps are transplanted from the shoulder or arm to the neck or thorax. When the flap is taken from any part of the face, a much narrower peduncle will suffice. The minimum of safety has not yet been determined. The medium of Langenbeck's operations, when the nose has been restored from the forehead, is 6 lines for the breadth of the peduncle; the narrowest was $4\frac{1}{2}$, the broadest 7. Probably it might be reduced to 4 if thought necessary or desirable. But it must be remembered, as Langenbeck observes, that the skin near the root of the nose is very firm and full of vitality, and that there is scarcely any other part where the formation of so narrow a nourishing peduncle could be safely practised.

The supply of blood would be insufficient if the peduncle were either too narrow or too tightly stretched. The reflux of blood would also be prevented by too great twisting, or by the pressure of the twisted sutures.

If either the flap or peduncle be too much stretched, the skin assumes a marbly hue, and it is only by loosening the sutures, lengthening the peduncle, and freeing the surrounding parts, that gangrene can be prevented. Too great twisting of the peduncle can be obviated by giving an oblique direction to the primary incisions.

When annexing the peduncle in its new situation, all stretching and compression must be avoided. The sutures on each side should not be opposite to each other, or the narrow strip between them would inevitably be compressed.

The effect of the best-executed operations is often marred by the contraction of the free edges of a flap. This is to be avoided either by uniting the free edge of the flap with neighbouring mucous membrane; by turning the free edge inwards, and so fixing the two raw surfaces of the flap together by suture, that the edge consists of a round duplicature of the flap; or thirdly, by reckoning for the degree of cicatrix contraction and making the flap too long.

The fourth method according to which plastic operations are performed, I have described as follows:—"By using some part of the body of another person, or some distant part of the body of the same person, which has been entirely separated, and retains no connexion with the person or part from which it has been removed."

Very wonderful accounts are to be met with in many books, of successful re-union of parts of the body which had been entirely separated. One man cut one of his fingers off; and, after a long search, found it among some saw-dust. It was then cleansed, replaced, and union took place. Another fished his finger out of a gutter, and the same fortunate result was obtained. A man bit off another man's nose, bruised it with his teeth, and spat it into the dirt. This also was successfully replaced. Another nose, thrown into the dirt, or, rather, the separated

point of it, was eaten by a dog, out of whose stomach it was afterwards cut; and this also united, after some hours' separation. Many such stories as these might be collected from the writings of men of good character and reputation. They are told sometimes with exceeding gravity, and with an amount of concurrent testimony which leads to a belief in the truth of some of the least marvellous, and suggests the inquiry whether success in such cases is to be considered as a lucky accident, or whether the retarded replacement, the apparently unfortunate delay, by which the wonder of the story is kept up, may not have been perhaps the cause of the fortunate result—whether the surgeon proceeds upon the best principles who at once sews on and binds up a separated part at the hazard of interposing a layer of coagulated blood between the two surfaces, and thus preventing their reciprocal contact; or how long a part, perfectly separated, retains its vitality sufficiently to become again a part of a living body, and at what period after the entire separation of a part of the body, or at what stage of the changes which take place in it, does reunion most readily take place?

Dieffenbach describes the change which takes place in a part which has been entirely separated as follows:—"If we cut away a piece of skin, which we have frequent opportunities of doing after surgical operations when parts are extirpated or amputated, we may notice a series of interesting phenomena, which show themselves still more clearly on the immediate separation of a flap of skin from the body. At the moment when the flap of skin is surrounded by the knife it becomes pale, not in consequence of loss of blood, but, when this is very small indeed, from a pure nervous affection, a spasmodic condition in the capillaries of the corium, which throws the blood out of it into the deeper parts. Very soon this pallor becomes less, even when the flap is perfectly separated. After the first bleeding from the edges, fresh blood flows, and this is repeated several times after its separation, until there appears a yellowish thin blood, and at last lymph. The edges of the corium are drawn together as if by a slight spasm, and the whole flap is slightly arched. These phenomena are most striking when the skin is from the scrotum; and, as I once took away a large portion of the scrotum, and applied it spread out, I found it afterwards wrinkled up together. Gradually life in the flap expires, and with its death all the above phenomena cease."—*Med. Times and Gazette, Dec.* 30, 1854, *p.* 661.

91.—ON DEEP-SEATED WHITLOW.

By John Hamilton, Esq., Surgeon to Richmond Hospital, Dublin.

[The following case illustrates an affection which has lately been very prevalent all over the country, and if the same deep incisions were made oftener, more fingers would be saved. This man was 40 years old, and was seized with a deep-seated whitlow. Deep incisions were

made in the front of the first phalanx, out of which matter gushed, to the man's great relief. Incisions were afterwards made in the palm and dorsum of the hand, but still the finger was obliged to be removed at the metacarpal articulation.]

The removed finger was carefully examined. The two last joints were black and shrivelled, and a faint line of separation was commencing between the dead and living parts. The first phalanx offered a good example of deep-seated paronychia in the advanced stage. The fibrous sheath of the tendon was very thick, the section white and firm, the lining membrane of a pink colour, and here and there bloodshot, with some deposits of flaky lymph. The flexor-profundus tendon was loose and soddened, and surrounded by pus. The periosteum separated from the bone by pus, and the bone dead. You will see here the contrast between this natural thin white sheath, with its smooth, pearly white lining membrane and glistening tendon, and the thick sheath of the whitlow finger with its rough, lymphy red inside, and its dull tendon smeared with pus.

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Many of you will think this subject scarcely worth your attention; but you are wrong; for you will find deep-seated whitlow a very serious and painful disease, by which the use of the finger or hand may be destroyed, and which requires prompt treatment; and nothing but a thorough knowledge of its nature and progress will lead you to insist early on those measures, which, though apparently severe, will alone arrest its destructive progress. As you have just seen, deep-seated paronychia is an inflammation of the inside of the sheath of the flexor tendons of the fingers, rapidly running into suppuration. The symptoms are usually as follows: the affected finger becomes painful, the pain increasing to the greatest degree, so as to prevent sleep, and often to cause the patient to spend the night walking about his room in torture. This will be at first accompanied by little swelling, and little redness, more especially in front, although the greatest suffering is complained of there. The finger will be kept bent, any attempt to straighten it increasing the pain, as does also pressure over the affected part. By keeping the finger flexed, the skin in front is relaxed, as well as the tendon, relieving the painful tension, and the pressure of the tendon on the inside of the inflamed sheath. The back of the finger next becomes red, swollen, and shining, pitting on pressure. You might be misled by this, and think it was the place to make your incisions in search of matter, but it will be found that pressure on this part can be borne, whereas in front, where Boyer says this whitlow always occurs, it cannot; and when the finger is attempted to be straightened, the suffering is at once referred to the front. After the third or fourth day, this part will be found swollen and prominent, particularly on a side view, but no fluctuation is discernible. If no treatment is adopted, the disease will often proceed from the finger to the palm, which becomes red, swollen, and tender, the back of the hand particularly so, of a deep shining red, pitting on pressure, and

fluctuation is soon apparent either over the knuckle of the affected finger, or in the web between the fingers. In the palm of the hand we often observe that the matter, after having made its way out through a small opening in the skin, does not pierce through the cuticle, which in the labouring classes is excessively thick, but separates it extensively, and you have a considerable collection of pus only covered by cuticle, before you come to the real abscess; as the palm becomes engaged other fingers get flexed, and cannot bear extension, and the pain shoots up the arm to the shoulder; in a woman, I knew it extend to the breast of the affected side. In most instances the inflammation with suppuration stops at the finger and palm, particularly when treatment has been resorted to; but if not, it may go up the front of the wrist to the forearm, in which case the patient recovers after weeks of suffering, worn out by sleepless nights and profuse suppuration, with bent, emaciated fingers, and a stiff wrist, and a nearly useless hand. Should the disease not extend so high, or not be so violent, it may end with a stiff bent finger. I should say when there has been suppuration the finger is invariably stiff, and usually bent, from adhesion of the integuments to the sheath of the tendons, and of the tendons to the inside of the sheath, while in the state of flexion. If the inflammation is very intense, the tendons die and protrude through the natural or artificial opening that has let out the matter, and throw off greyish white sloughs; where a portion of the flexor profundus is totally destroyed, the finger may be quite straight, being kept so by the extensor tendon. Such is the case in the man now in the house, who came to the hospital after having suffered from whitlow six weeks. I took away a dead portion of the flexor tendon, one and a-half inch long, from the sheath. I am going to remove his finger, as he presents a further step in the disease, viz., the periosteum is stripped from the bone of the first phalanx by effusion of pus, and the bone is killed.

The death of the bone is most common in the last phalanx, particularly of the thumb. In such a case you will find, though the matter has been let out by a free opening, the disease lingers, the part continues red and swollen, the opening discharges abundance of thin matter, and the large flabby granulations around it, and if you feel the end of the finger, there is a pseudo-fluctuating feel, as if it was extensively undermined; a probe passed in removes every doubt, by grating against the rough bone. It is best not to wait till it separates of itself, but enlarge the opening, and seizing the dead bone with a forceps, divide any ligamentous connexions at the joint, while any are on the stretch. The parts soon heal, the finger shortened, bent forward, and clubbed at the end, the nail irregular, but still a useful finger. In one of the coachmen in Mr. Champion's establishment, in whom the disease had been very violent for three months before I saw him, I found through a long side incision which had been made by a surgeon, the first and second phalanges dead and bare of periosteum,

and the third projected through a sloughy opening at the end of the finger; though the bone was dead, the soft parts were alive. In other cases both mortify, and the whole or part of the finger becomes cold, black, and shrivelled, as in Phelan's case, and also in a woman in Mr. Adams' ward, now in the hospital. Last season I had to remove a finger, for this cause, from a woman in whom it had mortified a few days after the disease began. Indeed, I have had to perform the same operation frequently, the cases being those in which the inflammation had been most acute, and the early treatment neglected. The excessive and rapid effusion into the sheath, and the attendant sudden swelling before the parts can accommodate themselves to it, seems to produce a strangulation, the circulation is arrested in the vessels, and the parts die.

The fever, in deep-seated whitlow, often runs high, and I have known delirium at night, but I never met with a fatal termination except in one case, a man who crushed his thumb while intoxicated; deep-seated paronychia followed, inflammation of the veins up the arm, and death.

On looking over the notes of many cases, I find the most frequent seat of deep-seated whitlows to be the middle finger, next the ring, and then the index finger, all before the thumb. This agrees with Boyer's experience; it differs from what we perceive in the superficial paronychia, in which the index finger and the thumb are so much the oftenest affected, and at their extremities. The part of the finger in the deep-seated paronychia most commonly engaged is the sheath of the flexor tendon over the first phalanx; this sheath is not only the longest, but the strongest and most complete. Men are more frequently affected than women, and the disease is most prevalent in winter. A few cases, when suppuration was threatened, subside and terminate by resolution. The majority run rapidly into suppuration, which is more or less extensive, according to the intensity of the inflammation. Such is an outline of the symptoms and pathology of deep-seated whitlow. You see that it is an inflammation of the inside of the sheath of the tendons, that nature makes vain efforts to get out the matter confined in the strong fibrous envelope; you have witnessed the disastrous effects of the unrelieved diseased action, the periosteum, the tendon, the bone, all at last engaged, and the finger, or even the hand, permanently injured or even lost. You will at once naturally arrive at the conclusion, that a free incision into the sheath, early performed, is the only treatment. Yet how is it that so many come to the hospital, where this has never been done, and all the destructive consequences allowed without an incision ever having been made.

I believe it arises from timidity, a fear of wounding blood vessels, particularly when the disease approaches the palm; there the palmar arch becomes a kind of bugbear. Some years ago, I made careful

dissections of the hand, with a view of furnishing myself with precise rules for making incisions in deep-seated whitlows. Permit me to call your attention to a few practical conclusions I arrived at:—

"There are two principal wrinkles or lines running across the palm of the hand; one, nearly transverse, corresponds to the joints between the metacarpal bones and first phalanges, and is situated about an inch behind the first transverse lines on the fingers. The second begins from the first, at the articulation of the index phalanx with its corresponding metacarpal bone, and goes in an oblique direction across the palm towards the pisiform bone at the inside of the wrist; *it answers pretty nearly to the course of the ulnar or superficial palmar arch.* These two lines which are very regular, will be found to serve as excellent guides in making our incisions in paronychia with confidence.

On the fingers there are three bundles of transverse lines; the two from the tip correspond to the articulations, that next the palm does not correspond to any joint, but is situated about the centre of the first phalanx and of the sheath of the flexor tendon.

The palmar fascia goes about as far as the transverse palmar line before it sends off its four processes which go between the fingers, and are attached to the sides along with the tendinous insertions of the extensor muscles and the lumbricales.

The web between the fingers contains the digital artery from the palmar arch and its division, and is filled up with loose cellular tissue, allowing matter easily to go from the palm to the back of the hand. As the superficial palmar arch, the only one likely to be wounded, corresponds to the oblique line which is behind the transverse line, the distance increasing as it approaches the inside of the wrist, you can, in all the fingers, *safely* make your incisions as far as the transverse line; nay, in all fingers but the middle, you can cut beyond this as far as the centre of the hand, and in the index finger even further. But it is worse than useless ever to carry your incision from the finger further than the transverse line, so as to endanger the arterial arch at all, as after going so far it is quite sufficient to pass a director under the palmar fascia, which alone confines the matter, if it has gone into the hand. On the director, you could, if desirable, cut out through the fascia, and even the annular ligament. Whereas if you cut down without a director to the palm, through all the parts to the bone (which in the palm is unnecessary, as there is nothing below the palmar fascia or annular ligament to confine matter), you could scarcely avoid wounding the arteries or large nerves. In the same way in the fingers after the sheaths of the tendons are divided, nothing remains to confine matter. These sheaths extend from the tip of the fingers to the transverse palmar line or metacarpal phalangeal articulation; they

are thick and strong between the joints, thin opposite to them; you should avoid over the joints, and confine your incisions to the strong part of the sheaths between them, cutting to the bone if you choose, or what is generally enough, down to the tendon, taking care to open the sheath well, and always keeping in the centre to avoid the digital arteries running along the sides of the fingers. In the thumb it is particularly necessary to observe this last rule, as, otherwise, after cutting beyond the first joints you would be very likely to wound the superficialis volæ.

Bearing these facts in mind, you will have no hesitation in making sufficiently free and deep incisions. I repeat, take care that your cut is long enough to open the sheath freely; a mere deep puncture through it may let out matter, and give ease for the time; but not allowing free vent, will cause the pus to seek another outlet, and much mischief ensue."

Three weeks ago, the Rev. Mr. Crampton's coachman came to me, suffering dreadfully from deep-seated whitlow in the sheath of the tendon of the first phalanx of the middle finger of the left hand. I plunged in a sharp, straight bistoury, and laid open, by continuing the incision, at least one-half of the sheath; a quantity of thick matter gushed out, and he never had any more trouble from the finger. You are not always so successful; in spite of free incisions the diseased action is not stopped. In such cases, though there is at first much relief, the unpleasant sensations return, the next night the patient will be kept awake by deep-seated throbbing pain, and you may be sure that the matter is working its way out in some other direction, and is not getting free exit, and you will probably discover some swollen tender spot, to repeat the incision. If the disease presents itself in a more advanced and aggravated form, not only engaging the finger, but extending to the palm, back of the hand, or even the wrist, which last is very rare, you will have to follow the matter, regulating your incisions by the rules I have mentioned. Some authors have made a separate division of paronychia, which they name paronychia periostei, from supposing the disease in this form to commence in the periosteum covering some part of the phalanges, with suppuration between the periosteum and bone, death of the bone, &c. I regard the affection of the periosteum rather as a secondary effect of the deep-seated paronychia, whose seat we have seen to be the sheaths of the tendons, but when neglected, to engage finally, all the tissues, the periosteum among the rest, and the bone. When the first or second phalanges are stripped of periosteum, and dead, it is best to remove the whole finger at the matacarpal articulation, as I am going to do with the man in the house, who has only the first phalanx bare and dead. If it is the last phalanx, it can be removed alone, and as I have already said, a useful finger will remain after the opening is healed.—*Dub. Hospital Gazette, Feb.* 15, 1855, *p.* 21.

92.—ON CARBUNCLE.

[A very valuable and lengthy report on this prevalent affection is published in the 'Med. Times and Gazette' for Dec. 2, 1854. The increase, in late years, of this disease has been considerable, and out of proportion to the increase of population. Everybody, whether delicate or healthy, seems disposed to be attacked. Diet would seem to be connected in some way with the disease. It is known that John Hunter cured himself of an habitual disposition to boils by a two months' use of carbonate of potash. We make the following extracts from the report.]

Premonitory Symptoms.—In 8 of the cases reported, no note as to premonitory symptoms was preserved. Of the 27, there were 9 in which the patient could not remember having felt in the least out of health, until the carbuncle appeared, and in one of these he had been previously feeling unusually well; in 3, a feeling of general lassitude had been the only symptom; in 1, headache; in 1, back-ache; in 1, "aching pains in all the bones;" in 1, liability to cramp had been observed. In the remaining 12 the introductory stage had been better marked. Drowsiness, headache, giddiness, and a feeling of extreme lassitude, sometimes with a tendency to stupor, had been its chief characters. In several instances these symptoms had been present for two months or more; in others for several weeks, and in one for eight days only. The urine had generally been observed to be high coloured, and to deposit sediment on standing, and in most the bowels had been costive or irregular. A gradual decrease of relish for food had been observed in most in which there were any premonitory symptoms; but, in few, had it amounted to absolute loss of appetite before the outbreak of the carbuncle.

For these symptoms the only authorised generalisation perhaps is, that they concur to indicate much functional disorder, and in all probability a vitiated condition of the blood. Whether produced by something nocuous taken from without, or by the retention of principles which should have been eliminated, the data are yet wanting to decide.

Part Affected.—No single part of the body could perhaps be named on which carbuncle may not occur. Instances have presented themselves of late years in the hospitals on almost every possible locality. The feet as far as our personal observation has extended have been exempt, but Mr. Coulson has mentioned to us a case in which he was recently consulted where the disease in a severe form was located on the dorsum of the foot. Its great rarity on the scalp is still observed. On the face, however, of late, the frequency of the carbuncle has been far greater than appears formerly to have been the case. When seated on the face, there is often present a more severe form of inflammatory fever, and the inflammation is less asthenic than in other regions. A series of most severe cases of this description occurred

about three years ago in St. Bartholomew's Hospital, and since then we have chanced on several other isolated examples of the same kind. A case in St. Mary's Hospital, for instance, recently mentioned in our "Statistics of Operations," ended fatally after herniotomy, from the outbreak of an acute carbuncle of the lip. We shall not enter at any further length on the consideration of this class of somewhat peculiar cases since it has already been minutely investigated in our pages. It was an observation of Sir Astley Cooper's, that when carbuncle attacked the head, it generally caused death. Though it must undoubtedly be admitted that the disease is by far more dangerous in this locality than in any other, yet recent experience does not support so sweeping a statement. In ten out of the thirty-five cases tabulated, the disease was seated either partially or wholly on the face, and the whole of them notwithstanding recovered. The favourite seats of carbuncle are those in which the skin is thickest; the nape of the neck, the back, and the nates, standing prominent in the list. Of the cases mentioned, in 12 it appeared in the nape, in 10 on the back, and 2 on the buttock. More extended, though less exact, observation, would lead us to believe that the proportion of cases in which carbuncle attacks the face is not nearly so relatively great as would seem from our table. Probably some allowance must here be made for what is mere coincidence.

Constitutional Symptoms attending the Outbreak.—The facts recorded in the columns referring to this part of the subject are intended to apply to the symptoms present during the time that the carbuncular inflammation was spreading. In very exceptional cases the disease was unattended by general disturbance, but in the large majority inflammatory fever of a more or less severe kind was present. In these the patient lost appetite, became heavy and depressed, had a somewhat furred tongue, a quickened pulse, costive bowels, and high-coloured urine. In some of the more cachectic or elderly patients the depression was severe, and the fever tended to what is familiarly known as "typhoid" in type. Some authors have asserted that the urine in carbuncle is generally phosphatic, and others that it is often saccharine; but neither of these conditions were observed in any one of the cases mentioned. The urine was always, when it differed from that of health, characteristic of inflammatory disease, being deficient in quantity, of a high colour, and generally loaded with lithates. It is but fair to attribute part of the constitutional disturbance present during the progressive stage of carbuncle to the extreme pain almost constantly present. This will not, however, account for the whole, and the greater part is doubtless symptomatic of the carbuncular dyscrasia.

Duration, &c.—The average duration of the disease, reckoned from the commencement of the inflammation to such time as the ulcer was either healed, or so nearly so that the patient was able to leave the Hospital, was, in the cases included in the table, nearly

seven weeks. The greater part of that period was of course occupied in the healing of the sore. Carbuncle does not appear to observe any determinate course as to the length of its stages. The time occupied in attaining its maximum size varied in the cases before us from three to sixty days, its average being rather more than fifteen days.

Nature of the Slough.—Rokitanski has asserted, contrary to the general belief, that the core of boils and the shreddy slough of carbuncles do not consist of dead tissue, but are inflammatory exudations like the false membrane of croup. Without at all denying that they may be in part so formed, we cannot avoid expressing an opinion that their bulk is really a slough. The circumstance that in carbuncle, after the removal of the core, the subjacent muscles, &c., are often exposed, their cellular tissues having evidently been destroyed, the toughness of the membrane, and its firm adhesion to the surrounding parts, all seem to support the old belief. Mr. Paget informs us that he has, under the microscope, seen areolar tissue in the core of carbuncle, and one or two examinations of the same kind made by the writer have convinced him of the same fact.

General Treatment.—Three or four different plans of treatment are in vogue in the Hospitals for the treatment of carbuncle. 1st. By stimulants, full diet, quinine, ammonia, &c. 2nd. By opium, freely used, and generally combined with tonics. 3rd. A more expectant plan, by salines, low diet, &c., in the first stage, and the moderate use of tonics afterwards. In all these, purgatives, of course, form a part. One of the largest carbuncles which we have ever seen occurred to a tailor, from Hampton Court, who was treated by Mr. De Morgan, in the Middlesex Hospital, about two years ago. The case was an extremely severe one, the depression of the vital powers being such that, for more than a fortnight the result seemed very doubtful. The very free use of opium, in combination with liquor cinchonæ appeared to answer an excellent end, and the man ultimately recovered. We have, however, not seen the opiate plan tried on a sufficiently large scale to justify an opinion as to its general merits, the method by stimulants being by far the most frequently adopted. Mr. Lloyd, at St. Bartholomew's, is the surgeon who is perhaps the most sparing of any in the use of stimulating remedies; and we certainly have not observed that his cases do any worse than those of others. In the early stages, Mr. Lloyd generally prescribes salines and fever diet; unless, indeed, the patient be very old or infirm. In one case under his care, after the sloughs were separating, several attempts were made to give stimulants, but on each occasion, a fresh accession of inflammation being caused, they were at length desisted from, and the man recovered very satisfactorily without. In carbuncles, as in all other diseases, the surgeon should doubtless pay less regard to the name and reputed pathology of the affection than to the symptoms actually present. If the system be much depressed, if the dis-

ease appear to have resulted from bad living, and, above all, if the patient's instincts demand them, stimulants should be pushed. If there be extreme restlessness, pain, exhaustion, &c., and if, after a few doses, it appears to agree with the stomach, the sustaining properties of opium may probably be very valuable. Should, however, the patient be of fair strength, the disease in its stage of advance, and attended by sharp inflammatory fever, it may be doubted whether any empirical evidence as the usefulness of stimuli under such circumstances has yet been obtained sufficiently strong to lead the surgeon to ignore the rational principles of treatment. In such a condition, the arrest of the disease would probably be much sooner effected by alteratives, purgatives, and salines, than by brandy or opium. It is even not improbable that the bleeding caused by incisions is often of great advantage.

Local Treatment.—From the time of Wiseman, the practice has been general of making free crucial incisions into all carbuncles. It has not, however, been absolutely universal. A few surgeons, among whom may be mentioned John Pearson in our own country, Von Walther in Germany, and Perrez, in France, have been sceptical as to its efficacy. There are also at the present day some surgeons who do not recommend it. Chelius, on the contrary, who appears to have paid some attention to this question, records thus, in strong terms, his conclusions :—"That cutting into the carbuncle is generally neither useful nor necessary, is the most dangerous statement that can be put forth respecting its treatment." Respecting this point, two propositions may, we think, be maintained: 1st. Carbuncles will very frequently get well without having been incised. 2ndly. The practice of incision, however free, does not by any means universally arrest the carbuncular inflammation. In proof of the former, the fact, that of the cases in our list, eight were not treated by incisions, is conclusive. In these, for the most part, the patients came under care when the disease was evidently arrested, and when free openings had spontaneously formed; so that there was no object for resort to the knife. Many others besides these have been under our observation, and, as a general rule, it may be asserted that the destruction of the tissues is far less extensive when the incision has been avoided, than when practised. The consequent cicatrix is of course much less also. The two following cases are of interest, because, in them, although under treatment in the stage of advance, yet incisions were purposely avoided.

Case.—A stout pale man, aged 50, by trade a baker, applied as an out-patient at the Hospital for Skin Diseases, with a carbuncle the size of an ordinary cheese-plate in his back. It had been a fortnight forming, and had been attended by great pain. In its centre, the depth of diseased tissues was probably about an inch and a half. Its edges were tense and tender, and the overlaying skin was livid

and congested. Several small openings had already formed near its its centre. The inflammatory fever, as may be judged from the fact of the man's applying as an out-patient, was not great; the disease indeed, although extensive, was not one of the most acute forms. For several nights the pain had been sufficient to quite deprive the man of rest. Mr. Startin ordered a mixture containing Epsom salts, colchicum, and sulphate of iron, and applied an acid solution of the nitrate of mercury very freely over the extent of about a shilling in the middle of the swelling. Two days afterwards, the man again applied. The disease was much as before, and had scarcely spread. The nights had been nearly sleepless with pain. The acid had merely caused a slough of the diseased skin to which it had been applied. A second application was made to the tissues beneath the eschar, and the general treatment was continued.

At his next visit the man was much better; the pain had all but ceased; the induration of the swelling had much diminished. From this time forwards the cure was rapid. Considering the size of the carbuncle, the amount of slough which came away was surprisingly small, and, as it escaped almost solely by the hole made by the caustic, the scar left was trifling.

The following case, which occurred under the treatment of Mr. M'Whinnie, in St. Bartholomew's, was of much more acute character than the preceding:—

Case.—A woman, aged 30, previously of good health, was admitted on July 15, suffering from a large carbuncle between the angles of the scapulæ. It was attended with severe throbbing pain and much fever. The integument was livid and brawny, and, in its most prominent part, several small ulcerations had already occurred. Mr. M'Whinnie ordered the free application of the acid nitrate over the whole inflamed surface, and prescribed a generous diet, with wine, and the exhibition, twice daily, of a draught containing the sulphate of iron and Epsom salts.

In the course of a few days, the skin, to which the caustic had been applied, sloughed and separated, together with a large mass of cellular tissue, and the base of the sore assumed a healthy appearance. A few days later, an extension of the disease, in a direction upwards, took place, and the woman's health, which had improved, again relapsed. The part now involved was very large, and the inflammation acute. Mr. M'Whinnie accordingly decided to employ the knife, and, at his request, free incisions was practised. It must be borne in mind, that the part to which the acid had been put was now in a healthy and healing condition. After the incisions, no further extension took place, and the woman made, under the use of tonics, &c., a rapid recovery. When the parts were healed, Mr. M'Whinnie directed our attention to the very great difference in the amount of scar where the caustic had been used, and where the knife. In the

latter, the skin was puckered up in a way which, had the part been an exposed one, would have been most disfiguring; while, in the former, the cicatrix was smooth, and comparatively insignificant.

There can be little doubt but that in the first and more successful of these cases the man would have been saved several days' pain by the practice of incisions, but it would have been at the expense of much more sloughing and suppuration. As to the relief of pain resulting from incisions, a glance at the columns of the tabular report will afford conclusive evidence. Almost without exception a free cut across a carbuncle gives immediate relief to pain; and even when the disease spreads afterwards the pain never again becomes as severe as it was at first. The following, as to immediate and perfect relief, is only an example of the ordinary course. Neither it nor either of the above are included in the table.

Case.—William B., aged 67, admitted into St. Mary's Hospital, under the care of Mr. Coulson, on August 9th. He had a carbuncle, the size of the palm of a hand, over the sacrum, which had been three weeks forming. The swelling was tense and hard, and the skin round it of dusky red. He had suffered intense pain. Mr. Coulson made a free crucial incision into the swelling, from which a moderate amount of bleeding occurred. The patient was from that moment more completely relieved of pain, and his recovery was rapid.

The assertion that incisions, even of the freest kind, do not always arrest carbuncular inflammation, will scarcely be held to need proof by any one who has seen much of the disease. If the proof be required, however, we may refer to the fact, that in five out of the twenty-seven cases in which incisions were practised, their repetition was subsequently necessary. Yet granting the occasional spontaneous curability of carbuncle, and the occasional failure of treatment by incisions, the case still stands very triumphantly in favour of the latter. The only drawbacks to their use are the amount of sloughing and the large scar; but to set against these we have the complete relief of pain in almost every case, and the fact that of twenty-seven cases on our list, in no fewer than seventeen the inflammation never spread after the first incision. This latter fact is one of great importance, for it cannot for a moment be held that in so large a number the cessation of the diseased action was simultaneous with the incision treatment by accident. The relation between the two must, we think, be held proven; and the fact becomes more telling when we add to it, that of the six exceptions in which the disease spread, in none did the spreading continue more than five days, and in none were more than three incisions needed. Now, it happens, that a large majority of carbuncles occur in positions in which the avoidance of scar is a matter of no moment; and in those where it is important—the face for example—the disease is so dangerous to life that in its treatment considerations of personal beauty cannot be allowed much weight.

As to the employment of escharotics, there is this great objection, that if used effectually they must cause at least as much sloughing as the knife, while they do not much mitigate the pain. It cannot be assumed, that because nitric acid has been applied over a small portion in the centre of a carbuncle, and a cure results, that therefore the acid was the curative means. In all probability, the case referred to is an example of almost spontaneous recovery, as far as local means were concerned. In cases in which the spreading is progressing fast, and the danger to life is great, the treating of the cut surface with an escharotic would probably give much additional security of arrest. In the fearful cases of acute carbuncle affecting the face, such a plan would be well worth a trial. As to the form of caustic, the acid nitrate of mercury is a great favourite with Messrs. Startin and M'Whinnie, at the Hospital for Skin Diseases, and applied with a glass brush, is very convenient of use. At St. Mary's Hospital Mr. Ure has recently employed the potassa fusa for the same purpose.

Are Carbuncles eliminative?—It is well known, that after an outbreak of boils the patient's health often improves remarkably on what it was before. He gets rid of dyspeptic symptoms, of languor, of flying pains, &c., which had troubled him for months; regains a degree of appetite and of general vigour to which he had been long unaccustomed. The same often takes place after several other of the skin affections. They seem to be effective in eliminating something poisonous from the blood; at least such is the *primâ facie* explanation of the effect. Does the same kind of result at all generally follow carbuncle? The patients on whom the preceding observations have been formed, were all treated in Hospitals, and therefore ceased to come under notice as soon as convalescent; we can consequently add nothing as to their subsequent health. Speaking, however, from the experience of a few observed in private, and from the accounts given by patients who were admitted for other diseases, stating that they had formerly been the subjects of carbuncle, we should strongly incline to the belief that much improved health does usually follow that disease. The theory of elimination, however, must not be too hastily accepted. Is it not at least as probable that a carbuncle is simply an effect of a morbid condition, and is produced without reference to any consequent advantage? May not the improvement be fairly attributed to the medical care which a patient with carbuncle receives? In this way most severe diseases are, if the subject recover, actual benefits. A man who has been living inattentively, and in whose blood the elements of disease have been long accumulating, suddenly has an outbreak of erysipelas, of dyspepsia, of gout, or, perhaps, of carbuncle. He fasts, and is purged; his doctor gives him blue pill, and a good lecture on diet, and, when convalescent, sends him into the world again a wiser man, and with a degree of health to which he had long been a stranger. Unless something like proof of it were advanced, we need not here recur to any eliminative influence of the disease for explanation. Authors have mentioned

cases in which carbuncle suddenly subsided, and at the same time the patient sank, as proofs that the local disease was exerting a good influence. This is manifestly of little value in proof of such a notion. Phlebitis and pyæmia are known to be occasionally the modes of death in carbuncle, and very probably the cases alluded to were instances of their occurrence. It is well known that the original inflammation often rapidly subsides when pyæmia is established.

Summary.—The following appear to be conclusions deducible from the preceding facts:—

1. The frequency of carbuncle has vastly increased of late years, and still continues to do so.
2. But little is known respecting its predisposing causes.
3. It may affect any age, excepting perhaps the very young.
4. Men are much more liable to it than women.
5. It occurs without distinction, at all periods of the year.
6. It occurs in almost equal proportions among the temperate and well-fed and the intemperate and ill-fed.
7. It has a premonitory stage in a considerable proportion of instances.
8. Its general treatment should be by purgatives and alteratives in all cases, and by stimulants or salines, according to the character of the constitutional disturbance.
9. Incisions are demanded when a carbuncle is spreading, or attended by much pain.
10. In a great majority of cases, free incisions relieve the pain, and in a considerable degree arrest also the spread of the disease.
11. If the spreading and the pain have already ceased, no benefit will be derived from incisions, but the sloughing and suppuration will be much increased.
12. The "core" which separates consists, for the most part, of dead areolar tissue.
13. No proof exists that carbuncle exerts any eliminative influence on the system.—*Med. Times and Gazette, Dec.* 2, 1854, *p.* 567.

93.—*Treatment of Sloughing Sores by Chlorine.* By Thomas S. Fletcher, Esq., Bromsgrove.—[Mr. Fletcher calls our attention to the treatment of sloughing wounds by means of chlorine. The first case is related as follows:]

A man, aged 84, was in August last shot through the palm of the hand, and, in a few days, the wound becoming very offensive, each orifice was covered with large pads of tow, dipped in a weak solution of chloride of lime (two drachms of the powder to a pint of cold water,) and the dressing renewed about twice a day. The offensive odour was immediately destroyed, the wound became clean and healthy, the granulations sprung up with great rapidity, soon covering the ends of the fractured bones, and healing up the wound, and this without the use of any other remedy.

I have trusted to the sole use of chlorine in other cases with equal success, applying it not only where the wounds are become offensive, but in the earlier stages, where there was reason to apprehend there would be sloughing or offensive discharges. Various preparations of chlorine have long been used to remove the odour from wounds, and for this purpose they are applied over other dressings; but what I here wish to recommend is that it should be used alone, and as a curative remedy. The *rationale* of its action may perhaps be explained in the following manner:—Portions of the wounded parts are preserved from sloughing by means of the antiseptic properties possessed by chlorine; granulations are to a very remarkable extent promoted—first, by the stimulating power of chlorine, from its increasing the activity of the small vessels on the surface of the wound, and secondly, by the peculiar property it has of keeping the wound clean, and thus enabling the plastic cells formed from the liquor sanguinis to have immediate contact with, and adhere to, active living substance, and so form new healthy tissue, instead of their being thrown off in the form of pus. If chlorine has the power of increasing the process of granulation, it must, as a natural consequence, have in an equal ratio the property of suppressing the suppuration. Lastly, the formation of those noxious gases and larvæ so detrimental to the parts themselves and injurious to the general health, is prevented by the chemical action of chlorine.

Poultices, ointments, and hot and cold water applications, do not possess the above properties; and if they promote, as I fear they do, what is most favourable to sloughing, heat and moisture, and the confinement of the noxious gases and discharges to the parts, they must in these respects have a tendency to do harm.—*Lancet, Dec.* 9, 1854, *p.* 478.

94.—ON CHRONIC ULCERATION OF THE LEGS.

By Frederick C. Skey, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

[Perhaps there is not a more troublesome thing to manage than chronic ulceration of the leg in old people, especially among the poor and hospital out-patients. The following cases are interesting, as showing the wonderful effects of opium:]

W. G., aged 41, admitted under Mr. Skey into Harley ward, on Nov. 9th, 1854. On the left leg, on its inner aspect, was a large indolent ulcer, with very prominent white edges. He had been troubled with this for several years, and been unimproved by treatment. The ulcer was about the size of a crown-piece. He took the common diet, with a pint of porter daily, and soap and opium pill, five grains, at night. The alteration which even two doses of the pill made in the appearance of the ulcer was as remarkable as it was beneficial. The

granulations (before pale, flabby, and extremely unhealthy) were red, small, and covered with a thin layer of healthy pus; the edges, before unhealthy and white, were level with the granulations. This last change was, however, assisted by the application of strapping and bandage. He progressed favourably. The ulcer nearly healed, until Nov. 28th, when, with another patient in the ward, he was seized with phlebitis. The ulcer now rapidly lost ground. Under tonic and stimulating treatment he again got better, and is now in Abernethy ward almost convalescent.

G. C., aged 56, was admitted into Harley ward on the 9th of Nov., 1854. He had suffered for many years from numerous small ulcers on the left leg and foot, for which, as a means of cure, and as a last resource, amputation was deemed advisable. The ulcers, on admission, presented a most unhealthy aspect; deeply excavating; white, overhanging margin; pale, flabby granulations, from which a thin, sanious discharge issued. On admission, he was ordered the common diet of the hospital, and a pint of porter daily. Ordered, confection of bark, two drachms, three times a day. Acetate of morphia, two grains; simple cerate, a drachm,—as an external application to the ulcers. This treatment he continued till the 16th of Nov., when he took soap-and-opium pill, five grains, at night. This he continued till the 27th of Nov., when he began to take the pills three times a day, the effect on the ulcers being manifestly very good. He is now lying in Abernethy ward, gradually getting better, under the same treatment.

J. P., a healthy-looking countryman, was admitted into Harley ward, on the 11th of November, 1854, suffering from a chronic ulcer of the left leg, which had for some years resisted all attempts to cure it. The man suffered a good deal from cold extremities. The ulcer presented the ordinary characters of the indolent class. Good diet, rest, and the internal administration of opium night and morning, in the form of soap-and-opium pill, rapidly converted the unhealthy granulations into red-pointed and cleanly secreting ones. After a short time, (on Nov. 30th,) he took half a grain of extract of opium night and morning, as the combination with soap seemed to relax the bowels. An attack of phlebitis, from which, under the treatment mentioned in the ward-paper, (Dec. 1st,) he recovered, checked the process of cure (which was rapidly going on) for some days. On its cessation, the ulcer again assumed a healthy character, and he was discharged on the 4th of January.

Mr. P., a London merchant, aged sixty-six, tall and stout, had a chronic ulcer on his leg, nearly as large as the crown of a moderate-sized hat. It had been his companion for seventeen years. He applied to two eminent surgeons, who said that they neither could nor dared to heal it. The supposed evil attending the arrest of a long-standing wound in the system you must take for what it is worth; my faith is somewhat equivocal. I told him I could cure it, and I dared to cure

it. It discharged immense quantities of fœtid ichor, and had done so for years. Mr. P. was almost excluded from the drawing-rooms of his friends. I gave him half a grain of extract of opium night and morning, and I strapped his ulcer daily. In three days he expressed astonishment at the altered aspect of his deeply excavated ulcer. In a week, I increased the dose to one grain. In forty-two days, this immense wound had healed to the size of a small watch. His health improved under the treatment. During the above period, he had taken no form of medicine but that I have mentioned.

Such is the treatment I have adopted in these cases of ulcer of the lower limbs; and you will naturally inquire into its principles and objects, for it is not sufficient that you learn the efficacy of a given remedy in any given disease. You are bound to inquire into the rationale, or the principle, of a remedy, that you may hereafter yourselves extend its application, and generalize on its influence, were it for no other purpose than that of comparing, classing, or distinguishing diseases; for it may not unreasonably be inferred that two or more apparently dissimilar diseases, which are amenable to the same remedy, must have at least some characters in common.

I venture to attribute to this remarkable drug the property of promoting the formation of healthy granulations on a surface that, not withstanding all the previous appliances of surgery, is yet flat, pale, and ungranulating. Now, there is no example of the power of opium to effect this object, more conclusive, or in which there is more work to be done, than that form of disease of which I am speaking,—which consists of a gap formed on the surface of the body, of greater or less depth and diameter, and in which there exists not even a trace of a curative action,—and yet the object is accomplished by means of this agent, and often with remarkable celerity. We call opium a stimulant and a sedative. As a stimulant, it is not very often employed in practice; while its properties, as a sedative, are well known, and are in daily requisition. Its property of mitigating pain and of promoting sleep, is that for which it is almost exclusively employed, and so completely is its action associated with this sedative principle, that its occasional influence as a stimulant is almost entirely lost sight of, and the stimulating property has merged in the supposed sedative. How otherwise can we explain the reasoning of Mr. Pott, who may almost be termed the father of modern British surgery. In speaking of the subject of opium in the treatment of gangrena senilis, he refers its undoubted efficacy to its property of soothing pain. He says, "I have always found that whatever tended to calm, to relax, and to appease, at least retarded mischief, *if it did no more.*"

But, in truth, pain, though common, is by no means an invariable concomitant of senile gangrene, and it is tolerably notorious that opium is a valuable remedy in all cases of this disease. How then, on what principle, does opium act in those numerous cases of senile gangrene that are destitute of pain, and in which it is an equally efficacious remedy.

I believe that its sedative properties have little concern with the result. In truth, opium is a most valuable stimulant of the vital powers, and whether its action originate with the centre or the periphery of the circulation, whether primarily on the heart or on the capillary vessels, I do not pretend to know; but there is no drug, simple or composite, known to our pharmacologists that possesses an equal power with opium, of giving energy to the capillary system of arteries, of promoting animal warmth, and thus maintaining an equable balance of the circulation throughout the body. To maintain the balance of the circulation! How much of meaning is attached to these words! How many affections of the bodily frame may not be brought within the range of this definition! Take the common chilblain; what is it but a local congestion of blood caused by defective capillary power?—there is no better remedy than opium; cold feet, as characterizing a person or a constitution, equally relieved; senile gangrene, the result of arrest of the capillary circulation, or its apparent opposite, local hyperemia—these diseases, one and all, manifest a loss of local power, a failure in the balance of the circulation. The term "inflammation," a word formerly in the mouths of our professional brethren on all occasions, is now limited in its application, and should be yet more limited, and I believe, in a yet more advanced state of medical science, will be restricted to an actually rare condition of the system. The influence of opium in such conditions is that of promoting a genial warmth over the system, a glow exactly resembling, and in fact identical with, that produced by the reaction on the system which is caused by the cold-bath. *It is local health*, and the sensation is most agreeable. The benefit derived from opium, when administered for the purpose of arresting inflammatory action of the vessels, admits, I think, of much doubt, and should be resorted to with some hesitation as a remedial agent, though I am quite persuaded that the evil of its administration is greatly overrated. But who will profess ignorance in these days of the inestimable value of this agent when resorted to immediately after an attack of inflammation has been subdued by a local or general bleeding? Here we can imagine that, the activity of the disease being checked, the diffusing influence of opium on the circulation may act as a simple derivative, operating on the vessels at the moment they are not indisposed to yield up their blood, and to which indeed they are compelled by the diffusive power of the general stimulus.

Many years ago, and before the introduction of railway travelling, I was summoned late one afternoon to see a patient some 80 miles from London. I travelled outside the mail. This occurred in the month of December, and the night was extremely cold. By some mistake I omitted to bring my great coat; and, for the first hour, I suffered a good deal. On reaching a town at some 10 miles distance from London, I took the opportunity, while changing horses, to run across the street to a druggist's shop, where I ordered a draught, con-

taining twenty-five drops of tincture of opium. I believe I was the only person outside the coach that night who did not suffer the slightest sensation from cold.

But it will be urged by many, who have experimented on, and who have observed less than I have done, the medical properties of opium, the infinite importance of studying the reactive effects of this deadly poison, and they would inquire into the condition of a person so treated on the following day. You may be assured that it amounts to *nil*. You will, I am sure, readily understand what I mean when I say the cold and the opium mutually balanced and mutually neutralized each other. There could be no reaction, because the influence of the depressing agent—viz., the cold, rather than otherwise, exceeded in duration that of the stimulant. If the period of prostration were brief, and limited to one or two hours, the argument might hold; but it is but a sorry objection to be urged after all.

I wish I could impress on the minds of the medical authorities in the Crimea the real benefit that might be derived to our noble troops, beaten down by intense cold and suffering in its various forms, from the judicious administration of opium. If twenty-five or thirty drops of tincture of opium, in addition to his ordinary quantum of rum, were administered to each soldier whose nightly services are required in the trenches or on guard, you would hear little complaint of cold for that night, neither would it produce the smallest tendency to sleep. And what do you imagine would be the objection urged against the proposition? "You would destroy the efficiency of the entire army; you would corrupt their morals; you would engender the most enervating habits; they would all degenerate into professed opium-eaters; and, in fact," say the alarmists, "the idea is preposterous." Here again I assert that no possible evil could result; the only sensation, present and future, would be the absence of cold. If cold beget suffering, opium is the antidote to that suffering, and the one will assuredly neutralize the other.

Notwithstanding the prejudices and the bigotry that have long beset the public mind on this subject, and from which our profession is not totally exempt, there is no comparison to be drawn between the practice of dram-drinking and the excessive indulgence in the use of opium.

The man who indulges in spirituous liquors makes daily inroads on his digestive powers not less than on his brain. His appetite is destroyed, and the pabulum for his blood is withheld from his circulation. He is stamped for life, and his perfect health is irrecoverable. The influence of opium, when taken as a means of indulgence though deleterious, is not permanently injurious. It exercises no serious influence on his digestion or on his cerebral organs, and, the practice once controlled, leaves him in a condition to regain, without difficulty, the fullest vigour of both bodily and mental health.

I have related to you the particulars of several cases of chronic ulcer in which recovery was attributable to the medical properties of opium, and almost to opium alone. The character of these ulcers strongly marks the inactivity of their nature, and hence the class of society to which they belong. They are marked by a flat base, which indicates, by its pale, flabby uniformity of surface, that no reparatory action has approached it. It is often surrounded by a thick, high ridge of lymph, covered by unhealthy integuments. The depth of the ulcer, which may be seven or eight lines, is caused partly by the ridge, and partly by the excavation of the ulcer below the natural level of the healthy integuments. So long as this ridge exists, although granulations may form, and will form, from the date of the employment of the opium, yet cicatrization will never complete the process of cure unless the wound or ridge be absorbed. Now the action of opium is not alone exhibited in the development of healthy granulations, but in the entire complement of such actions as are required by the sore, viz., the formation of new material, and the absorption of the old.

The influence of the stimulant is exhibited therefore not on one particular function. It does not merely promote secretion, but it stimulates to healthy vital actions in their entirety—viz., secretion, organization, and absorption contemporaneously; the granulations are secreted and organized, while the circumvallation of unsound material, the product of years of growth, is gradually absorbed and reduced to the level of the surrounding integument; for the removal of this wall is quite as indispensable to the ultimate result as the obliteration of the cavity by granulation. Without the two surfaces be brought to the same level, the process of cicatrization, or skinning over, will never be perfected.

If, therefore, we find that a disease like that I have described, and which exhibits so palpably a dormant condition of the remote capillaries, is amenable to this form of stimulant, which can only accomplish the cure by the substitution of healthy for morbid actions, why should we restrict its employment to this class of diseases? Why, as I have elsewhere inquired, may we not experiment with success on any local disease dependent on the same cause—viz., an inert condition of the remote vascular system?

In claiming for opium the merit of rousing into healthy action the dormant capillary system, to the end of accomplishing the permanent cure of chronic ulcer of the legs in old persons, I by no means wish you to infer that I consider all other modes of treatment unworthy of trial. Indeed, I attach great value to that recommended by Mr. Baynton, of Bristol, and others; but, having tested their value, I have no hesitation in pronouncing that which I have recommended, so far as I am competent to judge, as by far the most certain and efficacious.—*Lancet, Feb.* 3, 1855, *p.* 117.

95.—TREATMENT OF ERYSIPELAS.

By M. Velpeau, Paris.

[M. Velpeau gives us the result of his treatment of 1000 cases of erysipelas. He seems to place the greatest faith in iron.]

Calling to mind the modifications which the preparations of iron produce in the blood, it seemed to him that a disease so superficially placed, and one in which the inflamed tissues are so imbibed with altered fluids, was well calculated to be influenced by ferruginous preparations. He employed the *protosulphate of iron* in the proportion of 30 *grammes* to the *litre* of water (℥ viiss. ad ℥ xxxv.), or 8 parts to 30 of lard. In forty cases in which this was tried the erysipelas yielded in from twenty-four to forty-eight hours. It is, however, remarkable, that when thus extinguished at its point of departure, it will still spread beyond this, along parts already imbibed with the iron. Whether the inflammation, in order to undergo modification, requires to become fully developed, and whether the remedy is merely curative without being preventive, further researches must show. More easily employed to some parts, the ointment would be preferable; but it is somewhat less efficacious than the lotion. When used, in should be applied three times a-day to the erysipelatous patch, and some way beyond its margin. The lotion should be applied by means of compresses, which are to be kept on with bandages, and wetted every few hours, so as to keep the skin always moistened. Thus far the remedy has never failed in cutting short the erysipelas: but it has a disadvantage in iron-moulding the linen.—*Bull. Thér.*

M. Débout, in allusion to the local application of sulphate of iron in erysipelas, recommended by MM. Velpeau and Devergie, states that the following formulæ are of approved value:—

The Ointment.

Sulphate of iron	5 to 10		parts.
Water	12½ „ 25		„
Oil...	12½ „ 25		„
Lard	70 „ 40		„
	100 . 100		

The Solution.

Sulphate of iron	10 to 20	or 40	parts.
Water	120 „ 110 „ 90		„
Glycerine	70 „ 70 „ 70		„
	200	200	200

Bulletin de Thérap.—Med. Times and Gazette, March 10 and 24, 1855, pp. 239, 289.

96.—*Treatment of Acne Rosacea.*—[Sulphur has lately been very efficacious in this troublesome and ugly eruption. Mr. Erasmus Wilson's plan is perhaps the best.]

A drachm of camphor is pulverised with alcohol; twice as much of milk of sulphur is then added, (Mr. Wilson recommends sulphur sublimatum;) afterwards distilled water, to render it sufficiently liquid for use. This lotion is smeared with the finger over the face freely at night, and more sparingly in the morning; the effect is generally very soon apparent, and is often most striking. The lac sulphuris often contains as much as sixty per cent. of sulphate of lime; this does not appear to interfere with the action of the remedy. This preparation is preferable, on account of its minute subdivision, which makes it less irritating, and also for its whiteness. The use of this lotion should be persisted in for a considerable period if the blotches return; and if the surface be thus preserved in a healthy state, Nature will gradually restore the deeper-seated structures to their normal condition. It does not appear to lose its efficacy by continued use.—*Lancet, March* 3, 1855, *p.* 237.

97.—*Treatment of Condylomata.*—The local remedy usually employed by Mr. Lloyd, in St. Bartholomew's Hospital, for the treatment of condylomata about the anus is calomel. Of this a pinch is taken in the fingers, and sprinkled over the diseased part, the application being repeated once in every two or three days. Mr. Wormald, we observe, uses the grey powder for the same purpose; and in the case of children, the latter is probably the safer application. For out-patients, whom it may not be deemed safe to trust with the calomel, Mr. Lloyd is accustomed to prescribe the following lotion:

℞. Hydrarg. chlorid. gr. viij.; zinci oxydi gr. viij.; mucilagin. ℥ij.; aq. ℥vj. Ft. lotio.—*Med. Times and Gaz., April* 14, 1855, *p.* 363.

98.—*Benzin or Benzole as a Remedy for Animal Parasites.*—Our readers are probably aware that benzin or benzole is a clear, colourless fluid possessed of a pungent etherial odour, which is produced by the decomposition of benzoic acid, or other organic substances, at a light temperature. It was long ago ascertained by Milne Edwards that its vapour was very fatal to insects. This property has led M. Reynal, of the veterinary school at Alfort, to employ it for the treatment of pedicular maladies among animals. He has found that it destroys the parasites in these diseases, more surely, and with more safety to the animal, than tobacco-juice, mercurial ointment, or any other of the many remedies used. It destroys the epizoa without at all injuring the skin.

It is proposed to use this fluid in the parasitical diseases of the human skin, especially in pityriasis or morbus pedicularis, and in scabies, &c.—*Bull de Therap.—Dub. Hospital Gaz., Feb.* 15, 1855, *p.* 32.

AFFECTIONS OF THE EYE.

99.—TREATMENT OF STRABISMUS.

By T. Spencer Wells, Esq., Surgeon to the Samaritan Hospital.

[A squinting patient goes to his surgeon and wishes to know if he can be cured of his defect. The surgeon answers that he can. But he too often forgets that *two* operations are required, and if he does not state this beforehand, the patient looks upon the first operation as a failure.]

How are you to know beforehand, whether a second operation will be necessary? Nothing can be more simple, if you bear in mind certain rules. You must regard the degree of mobility of the eyes, the difference of visual power, the amount of convergence, and the alternation of distortion on closing one eye and calling the other into action. In some cases, you may have but slight distortion, yet the motions of the eyeball are very much impeded. Such a case is not likely to be perfectly cured by operating even on both eyes. In almost all cases you will find a great difference in the visual power of the two eyes. The distorted eye is the weaker. In all the cases we have lately seen, this has been very marked. In one woman, the distorted eye was perfectly useless; she could not read ordinary type at all with it. It only served to render vision indistinct, so that she always closed it when threading a needle, or looking intently at an object. You take a book, and find that a patient can read with the straighter eye at a distance of two feet, while she cannot read with the other, the straighter one being closed, at a distance of six inches. In other cases, the difference is not so great, but there is almost always more or less. Then, as to the amount of convergence: if the distortion be slight, the degree of mobility not diminished, and the visual power of the two eyes nearly equal, you need not operate at all. You can cure the case by making the patient wear prismatic spectacles. I may enlarge on this more hereafter, in the meantime referring to a paper of mine on the subject in the 'Med. Times and Gazette' last year. (See 'Med. Times and Gazette,' vol. vii., p. 216). If you find the vision of both eyes good, but the degree of convergence considerable, in all probability two operations will be required, and you had better prepare the patient for it. If, with a considerable degree of convergence, you have, as I have said you almost always will have, very unequal visual power in the two eyes, the general rule is, that one operation will be sufficient; but, to be on the safe side, in forming your prognosis as to a second operation, you must determine the power you have of producing an alternation of distortion. You know that cases of strabismus are distinguished as alternating and non-alternating. In the one case, when both eyes are open, the same eye

is always distorted. In the other, the distortion is observed now in the right eye, and now in the left, although more frequently in one than the other. In the great majority of cases, by the closing the better eye, the distorted eye becomes straight, and reverts to its abnormal position as soon as its fellow is opened again. If you raise the closed lid of the straightened eye suddenly, you will probably find that the eye is distorted, but it becomes straight as the opposite eye returns to its old position. You have produced an alternation of the strabismus by closing the better eye, and calling its fellow into action. You may do the same thing by curing one eye by operation, and, in that case, may have to perform a second operation; and, if the distortion you produced was very decided, you had better prepare the patient for the *probability*. I do not say more, because you will often find that, after a few days, a distortion of the straighter eye which has supervened after operation disappears spontaneously. You will find it laid down as a rule in some books, that when the distortion shifts to the better eye after operation, both eyes should be operated on at once; and some surgeons go so far as to say, that if after having thoroughly divided the faulty muscle in one eye any distortion is apparent *in either* in looking straight forward, the second eye should be operated on without delay. My experience has convinced me, that these are serious practical errors. If you acted upon these principles, you would often perform a second operation quite unnecessarily, and would run into great danger of converting a convergent into a divergent squint, for which the patient would be the reverse of grateful. I have seen more than one case in the practice of other surgeons where the internal rectus of one eye has been divided; the eye not becoming straight, the corresponding muscle of the other eye has been cut at once, with the effect of causing divergent strabismus in one case immediately, and in others within two or three days. I would say, therefore, never perform your second operation until you have had time to observe the effect of the first. You would be *tolerably* safe if closing the sound eye still caused the eye operated on to become straight; but it is better to be *quite* safe, and I advise you, therefore, always to wait, rather than run the slightest danger of leaving your patient worse than you found him.

It is curious and interesting to observe how very rapidly the vision improves after division of one of the muscles of a squinting eye. In some cases the improvement is immediate. This was first pointed out to me by the late Mr. Adams, who wrote some papers on what he called muscular amaurosis. I never agreed in his opinion, that compression of the optic nerve by the recti muscles was the cause of impaired vision; for, looking to the anatomical arrangement of the nerve and muscles, I do not see how this compression could be exerted. I think it much more reasonable to believe that the muscle which produces distortion alters the form of the eye-ball, or makes such unequal pressure upon it as to alter the natural relations be-

tween the cornea, lens, vitreous humour, and retina, so that the rays of light are not refracted in the same degree or with the same regularity as they are in the healthy eye. Admitting this, we can understand the instantaneous improvement of vision we often observe as soon as a muscle has been divided, and the gradual improvement still more frequently observed as the eye assumes and retains its normal position.—*Med. Times and Gazette, Nov.* 4, 1854, *p.* 460.

100.—*On a simple Method of Performing the Operation for Fistula Lacrymalis.* By E. R. Bickersteth, Esq.—[It has hitherto been considered essential, that the point of the knife should pass into and be carried along the lacrymal canal, before attempting to introduce the style. Mr. Bickersteth has given this plan up. He says,]

I have ceased to operate as before, and now content myself by making a simple puncture into the abscess or lacrymal sac; and then introducing the style, guide it to the orifice of the canal, and with gentle pressure pass it along the tube. The ease and simplicity of this method of procedure is surprising as compared with the usual plan, and I believe it will be found very generally practicable, and equally efficacious in restoring the canal to a healthy condition. During the last five months I have adopted this simple means in six consecutive cases of confirmed fistula lacrymalis, with results at least as satisfactory as by the former system. In one case the fistula was of eighteen months' duration, yet the style passed without difficulty, and the patient made a rapid and complete recovery. In another case, the patient—a nervous and excitable female—could not bear the idea of being cut, I therefore insinuated the style along a fistulous opening, which fortunately happened to be nearly over the duct, and succeeded in passing it into the canal without trouble and without pain. She also made an excellent recovery, but an ugly scar remained from ulceration caused by the pressure of the style against the fistulous orifice, which was not quite over the canal.—*Monthly Journal of Med., April* 1855, *p.* 299.

101.—*Tarsal, or Meibomian Tumour.*—[This is one of the most common affections about the eyelids. Mr. Walton points out that it is an affection of the meibomian gland.]

Instances of spontaneous cure, either with or without suppuration, are very rare, and an operation is the proper measure. Now, as the nature of the tumour has not been the guide to the operative measures generally adopted, it is not surprising that some of the methods devised are abortive, and some require to be repeated. Dr. Mackenzie, in his work on the Eye, 4th edition, p, 154, says: "As it is unencysted, it is needless to think of a regular operation." Unless, when the tumour is opened, such means are adopted as shall destroy

the cyst, a cure is not generally effected; and hence it was, that Mr. Tyrrell, who well knew that there was a cyst, writes, "Unless the cyst be freely broken, the disease is apt to return; but, if well lacerated, adhesive inflammation soon follows." Mr. Walton operates differently, and endeavours to pull away, or dissect out, the cyst, as a more sure method still. The patient under consideration would allow but one of the tumours to be treated, and Mr. Walton passed the blade of a small scalpel through it, and divided the tumour and the integuments at the same time, and dissected away the cyst. The edges of the little wound were brought together with plaster. In reference to this treatment it may not be out of place here to quote a postscript which is appended to Mr. Walton's paper. It is as follows:—"I desire to say, that since I have adopted the pathological views above stated, I have, whenever surgical measures are required, ceased to employ in general the usual method of attacking the Meibomian tumour from the interior of the eye-lid; but, for the most part, that is, when the tumour takes an outward direction, I divide it on the outside, squeeze out the contents, and, when it can be accomplished, pull away the cyst with a pair of forceps, or, if necessary, remove it by dissection. I am enabled to assert, that this process is far superior to the other, insomuch as it is instantaneously effectual. It is not necessary for me to point out the tediousness and uncertainty of the older method. I must add, that there need be no fear respecting the formation of a scar on the eye-lid; for, if the incision be made horizontally, and the edges be brought together by a strip of plaster, no trace of the operation is left."—*Med. Times and Gazette, Dec.* 9, 1854, *p.* 593.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

102.—ON THE STATICS OF PREGNANCY.

By Dr. J. Matthews Duncan.

(Read before the Medico-Chirurgical Society of Edinburgh.)

In commencing, Dr. Duncan wished to guard the Society from supposing that in attaching numbers to angles, he wished to imply the existence of real mathematical exactness in the subject he discussed. These numbers merely expressed a near approximation to the actual condition. There were used in order to facilitate the application to the anatomical circumstances so expressed, of mechanical principles which can only be used in an exact way.

I. Position of the Uterus.—In the unimpregnated female, the uterus generally lies with its fundus near the centre of the imaginary plane of the brim of the pelvis, and having its long axis in a direction corresponding to the axis of the brim, that is, an imaginary line drawn at right angles to the plane of the brim from its centre.

The unimpregnated uterus, weighing about an ounce, is so unstably poised in its position, that the common description of it as *floating* there, is an extremely apt use of that term. In accordance with this, the first change produced by the increase of its weight and bulk in pregnancy is, that it sinks in the pelvis. Gradually increasing in size to dimensions greater than those of this bony ring, it rises out of it, and is described as coming to rest upon its brim. But the uterus cannot, without risk of misapprehension, be described as ever resting upon the brim of the pelvis.

It must be remembered that the brim of the pelvis is in the erect, the attitude understood in all such discussions, much nearer vertical than horizontal, being inclined to the horizon at an angle of above 60°, a position in which it is evident it can give no *support* in the ordinary acceptation of the term. If we turn to the body to be supported, we shall find its circumstances inconsistent with the above description. The long axis of the uterus is in all the earlier months of pregnancy inclined to the horizon at about the same angle as the axis of the brim, that is, an angle of about 30° to the horizon. In the latter months, the angle of inclination of the upper part, at least, exceeds 30° somewhat. From its nearly oviform shape, with the small

end lowest, and the nearly uniform density of its contents, it may be predicated that its centre of gravity is nearly the centre of its mass. A necessary condition for the support of any body is that its centre of gravity be supported. This necessary condition is not fulfilled to the pregnant uterus by the pelvis. Consequently the pelvis cannot be correctly described as *the* part supporting the uterus.

The pregnant uterus is supported on every side by the soft and elastic structures which everywhere surround it, as closely as if it were enveloped in a fluid. No single part can be described as *the* part supporting it. But with this qualification, the nearest approximation to a correct statement of the conditions of its equilibrium is the following:—It reposes upon an inclined plane formed by the interior abdominal wall, and is prevented from moving down the plane by the pelvis. To be at rest upon an inclined plane, a body must have a power exerted upon it to arrest its motion down the plane. In the pregnant female, this is the power exercised by the brim of the pelvis. The power required to resist the pressure down the plane is to the vertical pressure on the inclined plane, as the height of the plane to its length. Now, in an inclined plane, which rises from the horizon at about an angle of 30°, the pressure on the plane itself must be greater than the pressure in the direction down the plane in the proportion of 87 to 50. Thus it is found that the chief support of the uterus is the extensive, equable, soft, and elastic abdominal wall, not the comparatively limited, unequable, hard, and inelastic pelvic brim.

Dr. Duncan would not dwell at length on the application of this in explaining the production of pendulous belly, &c. He would allude only to the pains in the sides of pregnant women, which Montgomery has described as produced by the dragging upon the oblique muscles at their insertions. And the same circumstances he showed to afford an explanation why the gravid uterus had so little tendency to prolapse.

Dr. Duncan then pointed out that great misconceptions were entertained in regard to the position of the uterus in different positions of the female. For instance, in attempting to disprove the gravitation theory of the position of the fœtus *in utero*, it has been argued that that doctrine presupposes that the mother's body is in the vertical position, in order that the gravitation of the fœtal head may have the effect attributed to it, and that the horizontal position assumed during repose, or from other causes, ought to afford many more chances than the statistical results show, of the head assuming, by gravitation, other positions than the ordinary one. This argument, to have any force whatever, must imply that, in the vertical position of the female, the uterus is vertical, or nearly so; and in the horizontal or recumbent posture, horizontal, or nearly so. But this is very wide of the truth; nay, it is totally opposed to it. In the erect position of the female, the uterus is, in all the earlier months of pregnancy, not vertical, but inclined to the horizon at the moderate angle of about

30°. In the latter months of pregnancy, the uterus is generally, though not always, directed to an angle somewhat nearer half a right angle, by the resistance of the anterior abdominal wall to its growing in its earlier direction. In the supine position, we have found the uterus to be not horizontal, but inclined to the horizon at an angle greater than that in the erect attitude of the female. This angle is between 60 and 30°, probably much nearer 60 than 30°. In this way, it is evident that the uterus is more nearly vertical in the horizontal position of the woman, than in the erect. In both cases it is in an oblique position; and by this beautiful arrangement, the uterus is preserved, in an uniform condition, free from many statical variations which might otherwise affect it suddenly and injuriously. If the uterus were at any time vertical, then the lower part of the organ, the part least supported and perforated by the os, would be liable to be subjected to the pressure of a column of liquor amnii of about twelve inches in height, or equal to the longest diameter of the uterus. But by inclining the organ from the vertical position, the vertical height of the column of water is diminished, and with it the amount of the fluid pressure on every part of the walls of the organ.

Before leaving this part of his subject, Dr. Duncan would point out one practical application of it. In the third quarter of pregnancy, when the fœtus has acquired considerable firmness and weight, and before it has become at all closely adapted in form and dimensions to the uterine cavity, it may be made, by a very small amount of force, to bob about in the liquor amnii. Various positions of the female have been recommended as best adapted for successful repercussion or *ballottement*, as this bobbing of the fœtus is called. But a little attention to the configuration and attitude of the uterus will show that, on mechanical grounds, there is one position decidedly preferable to the others. This position is that of course where the operation can be best accomplished; in other words, where there is most room and freedom for the motion. In regard to the uterus itself, this is evidently its long diameter in a vertical direction. Its long diameter offers most space; and if in any other than a vertical position, motion in it would be impeded, not only by the liquor amnii, but also by the fœtus's own friction on the wall of the uterus to which it gravitated, and on which it would, to some extent, move. Now, in what position of the female is the uterus placed, with its long diameter vertical? He had already said that the organ developes itself forwards and upwards nearly in the direction of the axis of the pelvic brim in all the earlier months of pregnancy, at least. If, then, we place the brim, which, in the erect attitude, is inclined to the horizon at an angle of 60°, in a horizontal position, then the uterus will be vertical. For this purpose, the axis of the female, or at least the trunk, must be deflected to the same amount from the vertical line; that is, so as to form an angle of 30° with the horizon. This can easily

be effected, in actual practice, by propping the female with pillows on a couch.

II. Position of the Fœtus in Utero.—To give point to the description of the position of the fœtus, he would first state that it is often described to carry its body vertically, but with the head placed undermost; in other words, it is said to stand upon its head. To show that these descriptions are entirely wrong, it is merely necessary to observe that the fœtus at the full time is closely adapted to the cavity of the uterus, and to recall to mind what has been already said in regard to that organ. It will thus be observed that, in the erect and supine positions of the female, the fœtus, near the full time, lies obliquely to the horizon; that in neither attitude is it at all nearly vertical or horizontal, but in a position about midway; and that in the erect position of the female it is more nearly horizontal than in the supine. The mature fœtus is nearly horizontal in ordinary circumstances, only when the woman lies flat on the side. It is vertical, only when the trunk is inclined to the horizon at an angle of about 30 degrees.

From this position of the adult fœtus in the axis of the brim of the pelvis, several minor advantages might be pointed out as accruing to itself. But Dr. Duncan would merely observe that this fact is an important element in the mechanism of parturition; the fœtus being thus placed in the direction most favourable for the uterine efforts to overcome the most important mechanical difficulty of labour, the passage of the brim.

In the same way, as the nearest correct explanation of the equilibrium of the gravid uterus lies in the mechanical principles of the inclined plane, so these are similarly applicable to the case of the fœtus. As the uterus derives support on all sides from the organs environing it, so the fœtus is much more perfectly supported by the bland and warm liquor amnii, in which it neither *stands* nor *carries* itself, but reposes floating in almost perfect equilibrium. It is, however, specifically heavier than the liquor amnii. It therefore sinks in this fluid, and it does so with an almost unappreciable force. In accordance with the doctrines of the inclined plane, this minute force is subdivided between the plane on which it lies, the anterior wall of the uterus, and the brim of the pelvis, the latter bearing the smaller share. The same hydrostatical circumstances of the floating fœtus which relieve the uterus almost entirely from the pressure at particular points of a body which might obstruct its circulation, or otherwise injure it, do also, in the same degree, relieve the child from pressure which might be painful or irritating to it. By this beautiful arrangement, also, the cord is safe from liability to pressure which might be fatal to it. Again, the protection (says Dr. Arnott) given to the tender fœtus by the liquor amnii is such, that a blow from without is expended on the surrounding water and cannot reach the fœtus. During labour these same waters perform important functions: which belong, however, to hydrodynamics, not to hydrostatics.

It is here necessary to consider the statements of M. Dubois and his numerous followers, in regard to the position of the fœtus *in utero*, when not at or near the full time. These statements are founded upon statistics of the presentations of premature children in the course of miscarriage or abortion. The statistics show that the earlier the fœtus is expelled from the uterus, it is the less likely to present the head as the first part born. But, by an unwarrantable and quite unsupported step, they have been represented as proving the position of the fœtus *in utero*. These statistics are valuable, simply as indicating the presentation not before but only during parturition of premature children; and if such is the case, they are utterly valueless in the discussion of the question of the position of the fœtus before labour, or of its causes. The presentation in early labour of the fœtus, at or near the full time, is a very exact and trustworthy indication of its position before labour; and in many cases we can easily verify the identity of the presentation in labour with the part lying lowest before labour. But we have no evidence whatever that the presentation in a miscarriage or abortion is identical with the part lying lowest before miscarriage; and we have no means whatever of reasoning from the one to the other. On the contrary, the more round and globular shape of the uterus in the middle months of pregnancy, the non-adaptation of the child to the uterus, the great mobility of the child, and the greater abundance of liquor amnii in these months, are some of the mechanical elements in these early stages of pregnancy, which render it almost absurd to reason from the presentation of a premature child during birth to its position in the uterus, before the process of its expulsion commenced. There are no collections of ascertained facts in regard to the actual ordinary position of the fœtus *in utero* in the middle months of pregnancy. We can only reason in regard to it, from comparing the position of the fœtal heart with the shape and site of the uterus, from feeling in some cases the position of the limbs or of the head *in utero*, from the questionable results of *post mortem* examinations in these months, and from the statical knowledge which we can bring to bear upon it *a priori*.

Another specious argument, adduced against the gravitation theory, requires only a few words to shew its imperfection. If gravitation (it has been said) were the cause of the normal position with the head lowest, then this position should be found with more certainty when the gravitation of the head, from any cause, was proportionally greater than natural. In cases of intra-uterine hydrocephalus, the child's head is larger and heavier than usual; and sometimes it is excessively so. But this increased gravitation (it is added) does not render head presentations in these cases more common than usual, but the very reverse. Here the altered circumstances of a hydrocephalic fœtus have been altogether misapprehended. The hydrocephalic head, although truly much larger and heavier in air, is probably lighter and more buoyant in water. In this question we have to do with hydrocephalic children

only while immersed in liquor amnii. The larger the head is in these circumstances, the lighter and more buoyant it is; and the other extremity of the child is proportionally heavier. The fluid effused in hydrocephalus is specifically lighter than brain, and therefore renders the head more buoyant than it is under natural circumstances. It is also in all probability specifically lighter than liquor amnii, and consequently its accumulation will have a more decided effect in elevating the head. In this way it is demonstrated that the four times greater frequency of preternatural presentations in cases of hydrocephalic children, proves rather than disproves the influence of gravitation in deciding the position of the fœtus.

Dubois and others have attempted to shew that the position assumed by the fœtus when immersed in a fluid, or rather when allowed to fall through a mass of water, is inconsistent with the gravitation theory. It has been shewn, in these ill-devised experiments, that the part of the fœtus which reaches the bottom of the vessel first is not the head, as these authors fancy should be the case if the gravitation theory were correct, but is the shoulder. But the fact is, that the line of descent through a fluid of nearly equal specific gravity with the body descending is, in the case of the fœtus, as in all other bodies, not a vertical line in the long axis of the body, drawn from what is the absolutely heaviest part, but a vertical line from the centre of buoyancy to the centre of gravity. The same fact is expressed in the hydrostatical principle, that a body floating and immersed is in stablest equilibrium when the centre of gravity is vertically below the centre of buoyancy—*i. e.*, the centre of gravity of the fluid displaced. These very experiments, and numerous other facts, show that the centre of gravity is much nearer the cranium in the fœtus, and especially when in the fœtal attitude, than it is in the adult; that it is, in fact, near the upper part of the back, and nearer the posterior than the anterior surface of the child. The centre of buoyancy of the fœtus will be very nearly the centre of the oval figure which encircles the fœtus. Both these centres will be liable to constant variations with the varying dimensions and specific weights of different parts of different fœtuses, and with the varying attitudes of the parts of the same fœtus. But allowing for every variation, it is evident that if these two centres be in the case of the fœtus placed in the same vertical line, the fœtus will be inclined obliquely to the horizon, as we find it to be in its ordinary position. The experiments of Dubois, involving, as they do, rapid motion in falling through water, are fitted to afford results of a very small degree of reliability. So far as they go, they point to the fact of the descent of the fœtus, shoulder lowest; or, to speak more correctly, following the shoulder. In an experiment Dr. Duncan lately performed with a fresh still-born adult fœtus, retained by threads in the fœtal attitude, he found it to poise itself in an oblique direction, head lowest, in a solution of salt of nearly its own specific gravity. This position evidently corresponds closely to the ordinary uterine position.

This argument against the gravitation theory, it thus appears, has been quite misapprehended; and a more correct investigation of the circumstances reveals to us here, as everywhere else, the beautiful arrangements of nature, whereby the fœtus reposes in its sojourn in the womb in that position which is easiest and securest to the mother and to itself—in a position, indeed, which requires no effort whatever on its part for its adoption and maintenance.

The only other objection to the gravitation theory, which is deserving of mention, is founded on the fact, that dead or putrid children present the head as the lowest part in labour less frequently than the living; and it must be admitted, that the death of the fœtus does not remove it from the agency of gravitation. From these propositions the conclusion is jumped at, that mere gravitation cannot account for this difference in the ordinary position of the putrid and the living, for it is evident that gravitation should affect live just as it does dead matter. But here it has only to be pointed out that the major proposition of the syllogism is an assumption, and that this argument is a reasoning in the dark. The assumption is one very highly improbable, namely, that a dead, or rather putrid child, is in the same condition, *quoad* gravity, as a living one. We know that in the adult, at least, the circumstances of death and putridity produce great changes *quoad* gravity. What the changes are in the fœtus in similar circumstances we do not know. Till we do know, we must be content without attempting to reason as if we did.

Before proceeding with correctness to argue from the fact of frequent malpresentations of dead children to the causes of the ordinary presentation of the head, we must take carefully into account not only the new circumstance of death in these cases, but also all other circumstances in pregnancies with dead children, which are, or may be, statically different from the circumstances of healthy pregnancy. Among these are prematurity, quantity of liquor amnii, the shape of uterus, and the shape of the fœtus. Dr. Duncan had already said that those who use this argument have not directed their attention even to the altered statical relations produced by death and putridity of the fœtus.

All the knowledge we possess of the position of the fœtus, after it has entered the second half of pregnancy, leads us to believe that its head ordinarily lies lowest. Before the seventh month, it is still capable of having its position *in utero* changed, by changes merely in the attitude of the mother, and probably it possesses the power of effecting temporary changes, at least, by its own unaided movements. But the fœtus is generally in a state of repose, and not producing motions in its limbs or body. In this state of repose, in a fluid of nearly its own specific gravity, it is impossible to conceive its maintaining any position but under the influence of gravity. Its position must at all times be mainly, if not entirely, caused and determined by statical circumstances. It is quite conceivable, that while still comparatively

free in the uterus it may, by virtue of its very easy mobility in the dense liquor amnii, change its position. If this occur at a time when its dimensions are beginning to approximate to those of the uterus, having overcome some resistance of the uterine walls by the force of its own muscular efforts, or otherwise, as by accidents to the mother, it may not gravitate back to its old and ordinary position; and thus a preternatural presentation may be produced. But the same circumstances which give great comparative force to the muscular efforts of the fœtus, render its position in the same degree difficult of retention, and will tend always to subject it to the influence of gravitation. The uterine walls are everywhere smooth and glabrous, and rounded; and the fœtus lies in this cavity with its legs elevated; circumstances which appear to render its maintenance of any position but that of gravitation a greater feat than ever was performed by a rope-dancer. With all the advantages of its new circumstances, the child after birth cannot assume or maintain any position. How much less could it be expected to do so in the uterus, and under circumstances so disadvantageous.

III. Position of the Pregnant Female.—The researches of Weber have shewn that in the erect attitude of both sexes in ordinary circumstances, the body is accurately balanced upon the two ilio-femoral articulations, the atlanto-occipital and sacro-lumbar articulations being in the same transverse-vertical plane with the ilio-femoral articulations; the knee-joint and ankles being also in it. This adjustment and balancing of the trunk upon the ilio-femoral articulations must, in the impregnated female, be gradually changed more and more till the full period of pregnancy arrives. At this time the weight of the entire gravid uterus and contents is superadded to that of the anterior parts of the body, which in the virgin state balanced the posterior parts upon the ilio-femoral articulations, the fulcrum of the pelvic lever. To restore the balance, a corresponding amount of weight must be added to that upon the posterior arm of the pelvic lever. The weight added in front is probably about from twelve to fourteen pounds avoirdupois; and to restore the equipoise a corresponding amount of the upper part of the trunk and of the head must be moved backwards, so as to increase the weight behind. These theoretical observations must have been frequently confirmed by every observant eye in actual life. It is then to be remarked that the protuberant abdomen is rendered apparently more so by the attitude of the woman being as if excessively erect. To the necessity for this new adaptation, the increased anterior development and weight of the mammæ will contribute. In small, and especially short-bodied women, this change is most evident; a circumstance easily explained by the greater prominence forwards in such of the uterine tumour, and by their not being able otherwise to adapt the trunk to the new circumstances, as can be done with greatest facility in tall or long-bodied women. Some few women, especially those last alluded to, are enabled to conceal to a certain extent their gravid con-

dition by not assuming this change of attitude, nature securing by a different mechanism the equipoise of the trunk upon the lower limbs. In the mechanism of the erect position, the centre of gravity must be vertically above the ilio-femoral articulations, or points of support. In the virgin, this centre resides in the lower part of the lumbar division of the spinal column. The addition of the weight of the gravid uterus and contents, in front, would move this centre forwards. This is prevented, however, in most cases by the corresponding motion backwards of the upper portion of the trunk. In some women, either from voluntary or involuntary causes, this last motion does not take place. The equipoise of the body upon the ilio-femoral articulations, or restoration of the centre of gravity to its position vertically over these points of support, must be otherwise effected. This can evidently be done only by moving forward the supports, so that they may be vertically under the new site of the centre of gravity, in this class of pregnant females. But, as the acetabula are fixed bony sockets, this can be done only by diminishing the angle which the pelvis forms with the horizon. In this diminution of the obliquity of the pelvis, the forward movement of the ilio-femoral articulations takes place, and the equipoise is restored without any backward motion of the upper part of the trunk. This change in the pelvis is analogous to that taking place in old age, when the forward stoop is counterbalanced by it. The same is also observed in youth, when a forward stoop is produced, as in the course of some affections of the vertebræ. Dr. Duncan had already pointed out that the attitude of the gravid uterus is inconsiderably changed by the woman shifting from the erect to the supine posture, or *vice versa*. But though this be the fact, as regards the mere attitude of the uterus, yet great changes take place in it, in regard to its liability to vascular congestion, and other phenomena connected therewith. When the woman is recumbent, the blood is easily returned to the heart from the uterine sinuses, and there is little danger of vascular engorgement taking place. When the woman stands the return of blood is not so easily effected, it having now to overcome the increased resistance involved in the change of the position of the vena cava from being nearly horizontal to being nearly vertical. Of this circumstance clinical illustrations abound in the practice of obstetrics. A woman of delicate fibre, and easily made to miscarry, may require to maintain the horizontal position for the whole, or a part only of the period of pregnancy. In some such cases the assumption of the erect attitude for the shortest time induces engorgement of the uterine sinuses, separation of the placenta, hemorrhage, and abortion, or miscarriage. Hemorrhage, again, is induced in some cases of ulceration and polypus, only by continuation in the erect attitude; in the same way colour often re-appears in the discharges after delivery, on the first getting up; and all uterine hemorrhages are aggravated by the erect attitude. To the same cause also are to be attributed many inflammatory affections of the uterus, and many of the uterine pains attending disease in this region.

Dr. Simpson had some years ago taken pains to refute the theory brought forward by Dr. Duncan, and which, notwithstanding the very interesting and able paper just read, he held to be incapable of meeting the many objections which might be urged against it. As the subject was a very wide one, he (Dr. Simpson) would begin with the last statements in Dr. Duncan's paper, viz., those with reference to the influence of position in restraining uterine congestion and hemorrhage, and in obviating their effects. In his opinion, Dr. Duncan had overlooked one very important circumstance, viz., the absence of valves in the vena cava and uterine veins, a most important item, as it was this that allowed the blood to gravitate without opposition. In lecturing on these effects of position and gravitation, such as Dr. Duncan had given them in his paper, he (Dr. Simpson) had always taught that this element, the absence of valves, was the primary and essential element in the consideration of the whole subject. As to the position of the fœtus, in a paper which he (Dr. Simpson) had laid before this society some years ago, he had advocated the influence of excito-motory or reflex action as the kind of vital mechanism by which the fœtus assumed and maintained its position. At that time he felt himself at a loss to explain some of the *residuary* phenomena, he believed that perhaps the old doctrine of gravitation might so far explain them; but as yet he was not convinced that the gravitation theory was the only key to the solution of the whole question. For example, in every 100 cases of head-presentation we had twenty-eight or twenty-nine in the third position of Naegelé, where the back and occiput of the child, instead of lying to the front of the uterus, lay to the back of it, and the position thus assumed was the very opposite to what we should expect on the gravitation principles laid down by Dr. Duncan. And that this position was not assumed by the child only a few days before delivery we had the surest evidence, as auscultation showed that it was maintained for weeks before. In Hunter's celebrated plate of his supposed natural position of the fœtus, which Dr. Duncan had exhibited to the society, we had the long axis of the head of the child lying in the right diagonal with the occiput anteriorly. This was a very rare position of the head, as Naegelé had only met with it in one in 1200 cases; but if gravitation were the sole determining cause of the child's position, we ought to meet with it as frequently as we meet with the head in the opposite diagonal diameter. The true reason for its rarity, Dr. Simpson believed, lay in the fact of the rectum occupying that diagonal, and the child in consequence was compelled by reflex action to displace itself and to take up its position in the other diagonal of the brim; the position assumed by the child being always dependent on the freedom with which it can place itself so as to be least incommoded by the irritation of such pressure and agency as were calculated to excite reflex motions in it. Again, we must remember, that the child swims in a fluid of nearly equal specific gravity with itself, and in which it moves with the

facility of a fish in water. Continuous motion never tires it, in accordance with the proposition laid down by Marshall Hall, that for voluntary muscles rest is required, but that the "spinal system never sleeps." Dr. Duncan's argument as to the death of the child not influencing its position, was founded in his (Dr. Simpson's) opinion on incorrect data. With living children the proportion of breech presentations was about one in sixty; but when the child had been dead for some days before birth, the proportion of these presentations was one in six. Hence the position assumed by the child was not a physical act, but one connected with life; it was lost with the loss of life, and was hence a vital act to that extent. But all the vital acts of motion are dependant on muscular contractions, and muscular motions in the fœtus are entirely reflex and spinal, not volitional and cerebral. Dr. Duncan had attempted to account for this frequency of breech presentations by stating that the statics of the child had been altered by the decomposition which had taken place after death. We must remember, however, that in the decomposition of the child the first change observed was an enlargement of the abdomen, which ought rather to maintain the head downwards than force the pelvic extremity upwards. In fact, after death it was subject to statical laws alone, and hence its new position. As to the argument from hydrocephalic fœtuses, Dr. Simpson still regarded it as a strong one in favour of the true doctrine not being gravitation. We had two fluids, the hydrocephalic and the liquor amnii, of nearly similar specific gravity, but Dr. Duncan forgot that the weight of the cranial bones from the enlargement of the skull had greatly increased; and yet, notwithstanding this increase in the weight of the cephalic extremity, instead of the proportion of presentations of the head being ninety-six in every hundred cases and the pelvic presentations one in sixty, we had the pelvic presentations occurring one in every six cases of hydrocephalus, or ten times more frequent; and the cephalic presentations correspondingly reduced in number. Dr. Simpson could not regard Dr. Duncan's observations on Dubois' experiments as very accurate. At the sixth month, Dubois found that the child presented equally often with its pelvic and cephalic extremities; and he (Dr. Simpson) held that the presentation shown by a fœtus when the os uteri opened was the best existing test of the former position which it had occupied in the uterine cavity before the os did dilate. There were certain cases, however, which in his (Dr. Simpson's) opinion decided the question. He referred specially to cases where there were twins, and those in which there was a recurrence of malpresentations in single births, and where the uterus, instead of its usual ovoid, was of a more or less irregular form. Were the position of the child solely dependent on gravitation, this irregularity of form should not interfere with the physical action of gravitation, and yet we found the frequent recurrence of preternatural positions in such cases, conclusively showing that gravitation could not account for them. The child resembled a decapitated frog,

which to escape irksome pressure shifts its position independently of any act of gravitation, and adapts itself to the figure of the containing cavity. Lastly, Dr. Duncan had misstated the argument from the natural position of the woman and from the effect of the reclining posture. Velpeau had, many years before, used the same arguments; we know, however, that women have been confined to the horizontal posture during their entire pregnancy, and yet the head presented. In such cases the child must have inclined to one side or the other as the uterus shifted, and the fundus uteri thus being lower than the os, and, accordingly to the reasoning adopted by Dr. Duncan, we should have had the pelvis and not the head the presenting part. Granting that in the erect position and in the reclining position, when the mother lay on her back, the uterine walls formed an inclined plane, with the os as the lowest point and the one towards which the head would gravitate, yet we know equally well that when the pregnant woman lies during sleep on one or other side, as she usually does, the fundus uteri is considerably lower than the os uteri, the inclined plane of the uterine cavity is reversed, and the child's position, if dependent on gravitation alone, would be nightly liable to change. The subject of position of the fœtus was, however, a very extended one, and would require much more time than could be given to it at such a meeting. He was not aware from the title of Dr. Duncan's paper that it was to treat of fœtal positions, and what he (Dr. Simpson) had now stated was, perhaps, not so relevant to the point as a little consideration of the subject might easily make it.

Dr. Duncan, after referring to the additional explanation of the increased liability to vascular uterine congestion in the erect attitude, from the absence of valves in the veins between the heart and uterus, said he had only a few words to say in answer to the points taken up by Dr. Simpson. The latter had referred to the researches of Naegelé, which showed that the fœtal head entered the pelvis in a considerable proportion of cases with the face lying forward, or anteriorly, instead of the occiput as was most frequent. Dr. Duncan could not see that this affected any position taken up by him. For he did not think that the position of the head in early labour was any certain indication of its position before labour began. The position of the head in the pelvis, in early labour, was determined, as Dr. Simpson said, by the shape of that bony cavity. Now, Dr. Duncan's researches had reference exclusively to the position of the fœtus before labour, and before its head entered the pelvis at all. Moreover, all he wished to shew, was, that the position of equilibrium of the floating fœtus was an oblique one as in the human female. His researches did not enable him to say whether in this oblique position the child should float with back lowest or one of the sides. Peculiar attitudes of the uterus or fœtus, or the position of the liver, might explain the positions of the fœtus *in utero* with its sides or front anteriorly. These questions had to be decided before any argument against their production by gravitation

could be admitted. Dr. Simpson had said that the spinal system never sleeps, and that the reflex nervous actions and resulting motions might go on so as to cause positions of the child. But Dr. Simpson forgot that he knew that the child was generally in a state of repose, not moving, and consequently that any argument about the possibility of constant, unwearied movements in the fœtus, was a vain argument, seeing we knew that such motions do not exist. Moreover, the fact of the fœtus being in a fluid, while it gave the child great power of motion, gave it also great instability. A man had great power of movement when immersed in water. But he was also excessively unstable and had little power to resist movement. In regard to the frequency of malpresentations of dead children, Dr. Duncan again pointed out that we did not know the altered statical circumstances of dead children. We must wait to ascertain these before we can decide whether their frequent malpresentation is the result of loss of life and power of motion, or of such possible altered statical circumstances. One fact was worth a cartload of arguments. Dr. Duncan had one to adduce. He had floated in a saline solution of its own density, a fœtus of the sixth month, which was born dead, and with the skin peeling. It floated head highest. In labour it presented the breech. This was the only fact bearing on the question which he or Dr. Simpson had to adduce, and it certainly told against Dr. Simpson, and in favour of gravitation. The death and putridity had so altered it, that it floated in the opposite way from a fresh child. Dr. Simpson spoke of the probable effects of swollen abdomen in putrid children. But this kind of hypothetical and assumptive reasoning Dr. Duncan thought unworthy of even getting any place in the question. Again, in reasoning about the uterine attitude of hydrocephalic children, Dr. Duncan did not wish to force any conclusion from the circumstances he stated. He merely destroyed the argument of Dr. Simpson and others, derived from the frequent malpresentation of hydrocephalic children. We must await the results of actual experiments to decide the position of equilibrium of floating hydrocephalic children. Till such experiments were made, neither he nor Dr. Simpson had any right to argue from suppositions as to what such experiments would shew. The increased weight of skull in these cases would, in Dr. Duncan's opinion, be probably counterbalanced by the increased density of the rest of the child's body, the lighter parts being removed by emaciation or atrophy. Dr. Duncan referred to the cases of recurring malpresentations as being exactly the kind of cases from which no arguments could be drawn, till we knew the whole circumstances of such cases. The one circumstance of deformed uterus producing these results, Dr. Duncan demonstrated, could be more easily explained by gravitation than by motions of the child. In alluding to the position of the pregnant female on her side, as making the uterus horizontal, Dr. Simpson forgot that then the fœtus was on its side as well, and *de facto* in a position of stable

equilibrium, not liable to move. Finally, Dr. Duncan wished it to be understood, that he was not discussing the whole subject of the position of the *fœtus in utero*, but only its statical relations, leaving its own motions out of view. These statical relations, however, threw light upon, and explained in an easy manner many phenomena which Dr. Simpson and others had referred to reflex actions in the fœtus. Dr. Duncan was sure the Society could not fail to see that if the Newtonian law was followed, of choosing always the simplest explanation of phenomena, the gravitation theory would, so far as he had carried it, have an easy superiority over any other that could be adduced.—*Association Med. Journal, Jan.* 5, 1855, *p.* 16.

103.—*Use of Uva Ursi instead of Ergot of Rye.*—Mr. Harris relates cases in which he has employed a strong decoction of uva ursi in accouchements, where the ergot of rye would ordinarily have been employed, and found its employment followed by vigorous pains, which soon caused the expulsion of both fœtus and placenta. Mr. Harris prefers this medicine to ergot of rye, inasmuch as it does not cause such strong contractions as the latter, which are so very painful to the mother and dangerous to the child.—*Virginia Med. Jour.—Monthly Journal of Medicine, April* 1855, *p.* 346.

104.—*Uterine Douches in Tedious Labour.*—Dr. Bourgeois believes that the tepid douche, so successfully employed by Kiwisch and others, in the induction of premature labour, is susceptible of beneficial application in a much wider field. Thus, he has used it with great advantage in simple inertia of the uterus; a single douche, of a quarter of an hour's duration, often sufficing to arouse the pains immediately and effectually. Cases of rigidity of the uterus, met with in too young or too old women, delivered for the first time, or produced by a premature rupture of the membranes, are treated in the same manner with success. It is also advantageously employed, but with less certainty of success, in spasmodic action of the organ, which is so distressing a cause of delay.—*Gaz. des Hôp.—Med. Times and Gazette, March* 31, 1855, *p.* 317.

105.—*On Injection into the Uterus in cases of Flooding.* By Dr. Septimus Wray, Brixton.—It is now many years since I first made trial of a practice to which the present communication refers, and as I have every reason to believe that it has been the means of preserving life in several instances, I now feel justified in recommending it most strongly to the profession. Many are the plans that have been proposed for the arrest of hemorrhage after delivery. In these

fearful cases we have been advised to use cold affusions, to inject cold water into the vagina, to compress the uterus externally, to introduce the hand, to give astringents, stimulants. &c. Amongst all these recommendations, the plan, to my mind, the most simple and direct, and, in my practice, by far the most effectual, has been omitted. I allude to the injection of cold water *into the cavity of the uterus itself*. By so doing a stream of cold is secured to the open mouths of the vessels themselves; all clots, &c., which prevent closure are washed away, and the organ, to a matter of almost absolute certainty, is stimulated to contract. There is neither difficulty nor danger in the procedure. I use a Weiss's syringe, and introduce the nozzle an inch or two fairly into the os uteri, holding it in position whilst an assistant works the piston. A common stomach-pump, or enema syringe, will serve the purpose well; and with those fitted according to the new plan with an elastic propellor, the accoucheur might manage the whole without assistance. If, however, his syringe require two hands to work it, he will require help, as it is absolutely necessary that one hand be employed within the vagina in keeping the nozzle of the tube in place. The stream may be kept up as long as deemed necessary; and, if the case be very urgent, it might be well to use vinegar and water in equal parts, instead of water only. In one protracted case of hemorrhage I used a solution of lunar caustic, in the proportion of three grains to an ounce of water, with perfect success. The old plan of injecting into the vagina is often ineffectual, there being, indeed, no security that any of the water ever enters the uterus itself; whilst that of cold affusion to the abdomen is yet more remote in its mode of action. Having been for nearly forty years in extensive practice, I speak from considerable experience in these matters. I am very sanguine that, were the expedient I recommend in general employment, life need almost never be lost from the distressing accident referred to.—*Med. Times and Gazette, April* 14, 1855, *p.* 361.

106.—*Fecundation.*—Dr. Martin Barry has repeated all his former experiments as to the ovum, and though denied by Bischoff, Wagner, and various other Germans, the English physiologist proves to have been true from the beginning. He has recently shown spermatozoa in large numbers in the body of the ovum.—*Lancet, Jan.* 6, 1855, *p.* 25.

107.—BLENORRHAGIA OF THE EXCRETING DUCT OF THE VULVO-VAGINAL GLAND.

By Dr. Alph. Salmon.

The author directs attention to an affection very common among prostitutes, yet little known to the generality of practitioners, namely, purulent hyper-secretion of the excretory duct of the vulvo-vaginal

gland, a disease first pointed out by M. Huguier, and described by him as the occasional source of blenorrhagia in the male. It is now universally known, that there exists on each side of the vagina, at the orifice, and imbedded in the labia, a glandular body, the duct of which, half an-inch long, opens just at the base of the hymen, or by the carunculæ myrtiformes. During erotic excitement, a viscid fluid, similar to the prostatic fluid in the male, is abundantly poured forth to lubricate the female external organs. That the duct of this gland may become the seat of one variety of blenorrhagia is shown by the following cases:—

A girl, aged 18, named Heloise, came from Paris for the purpose of prostitution, and was immediately subjected to medical inspection. There was nothing morbid in the urethra, the vagina, the excretory duct of the gland, nor in the anus, but there was a slight ulceration of the neck of the uterus, which the author cauterised; he then kept the girl in the Hospital some days. She was subsequently re-examined, pronounced sound, and discharged. There was the same freedom from disease upon two successive examinations, at intervals of ten days; but shortly after the last visit she was marked as having infected a young man of respectability with a profuse blenorrhagia. Dr. Salmon examined her with great care, but found no disease in the urethra, vagina, or anus. Upon investigating the condition of the vulvo-vaginal gland, he noticed, upon pressing it with his finger from behind forwards, from the ischium towards the carunculæ myrtiformes, that there flowed from the orifice of the duct a moderate quantity of thick yellow pus; there was also a well-marked increase of volume in the part. This condition remaining unchanged for several days, the author injected a solution of nitrate of silver into the gland by means of one of Anel's syringes. No improvement ensuing after a fair trial, the orifice of the duct was cauterised by the tincture of iodine, applied by means of a fine bougie. This plan was continued for four days; the discharge became gradually less, and the patient was discharged cured 25 days after her admission.

A girl, named D., had been living in a reception-house at Chartres 15 days. She came from the environs of Rambouillet. A soldier pointed her out shortly as having infected him with blenorrhagia, when she was subjected to examination, and found to be suffering from this inflammatory condition of the vulvo-vaginal gland. After a visit of a fortnight to the Hospital, during which time she was subjected to proper treatment, she was dismissed cured. Several other similar cases are recorded, from the consideration of which the author arrives at the following conclusions:—

1. Blenorrhagia of the duct of the vulvo-vaginal gland is more common among young prostitutes who have just commenced their mode of life, than among those who have been long accustomed to it. All the cases recorded by the author were those of young women who had recently been admitted into the receiving-houses.

2. This form of blenorrhagia mostly affects the left side. Out of eight patients, the left gland was affected in six cases. The orifice of the duct is more easily observed on this side. The reason for this preference is not clear; for one cannot accept the explanation of Huguier, that it is due to pressure upon the left iliac vessels by the sigmoid flexure of the colon distended by fæces.

3. It is written by authors who have treated of abscess of the labia-majora, that excess of coitus is often a cause of the malady. This statement is verified by the fact, that the house which rendered the greater number of the cases related in Dr. Salmon's paper was much frequented, and its inmates, mostly young, were subjected to frequent and daily intercourse.

4. Blenorrhagia of the duct of the vulvo-vaginal gland is very common. It may exist without the coincidence of blenorrhagia of the urethra and vagina; it becomes the cause of an analogous affection in men. Eight clear cases in verification of this statement have been witnessed in the town of Chartres. In all, this form of blenorrhagia of the female existed alone; in two cases, attention was first directed to the girl by the complaint of the men who had become infected.

5. It is most important that medical men should turn their attention to this point in the examination of females.

6. The signs by which the disease may be known are derived from examination only. The woman experiences no inconvenience, nor does she think that she needs medical aid. Moreover, she may, if she please, conceal her disease, by making water, or by using as an injection a strong solution of alum, shortly before the visit of the Inspector. This discharge of pus may be easily overlooked, owing to the narrowness of the duct. It is necessary that the surgeon should first make moderate pressure of the labia against the rami of the ischia, by the thumb applied in front of the anus, that it may be ascertained whether the gland be tumefied. In the natural state, it cannot be detected by the touch; if swollen, it feels like a rounded body, the size of a nut, or larger. Firmer pressure made against the ischium from within will cause the contents to escape. The normally constituted fluid is thick and clear, and appears at the extremity of the duct in not larger quantity than a drop or two. The fluid from the inflamed gland is either thick, more abundant, and mixed with mucus, when the girl should be put under surveillance; or yellow and puriform, when she should be removed to an Hospital.

7. The treatment resembles that of blenorrhagia in other situations. The result is much more rapid. Absolute rest; injections of nitrate of silver, with Anel's syringe; cauterisation with the tincture of iodine; baths. The duration is about twenty days.—*L'Union Médicale.*—*Med. Times and Gazette, Dec.* 23, 1854, *p.* 646.

108.—ON THE EXCISION OF LARGE PEDUNCULATED UTERINE POLYPI, AND ITS ADVANTAGES OVER DELIGATION.

By Dr. J. Y. Simpson, Professor of Midwifery in the University of Edinburgh.

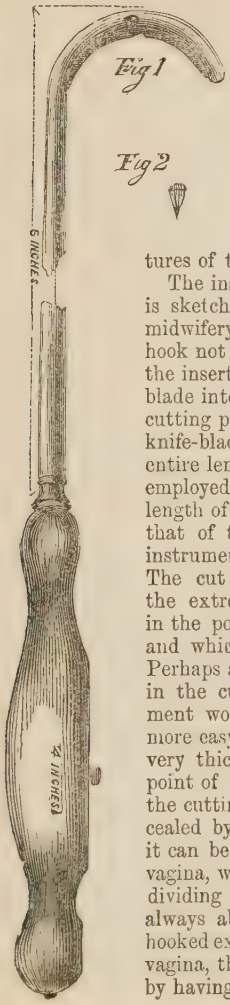
[As soon as polypi have passed the os uteri, they easily come within reach of surgical interference. Some surgeons prefer deligation and others excision. Dr. Simpson says:—]

The removal, however, of large uterine polypi by excision, has long appeared to me to be in many respects a simpler and a safer operation than their removal by ligature. It is, on the whole, more easily accomplished; the cure of the disease by it is infinitely quicker; it is accomplished with far less restraint and annoyance to the patient; with less risk of local irritation; and, as I believe, with less ultimate chance of actual peril to health and life.

Two objections have usually been urged by accoucheurs against the removal of large uterine polypi by excision. The first of these objections, viz., the danger of hemorrhage, has been particularly insisted on by those practitioners who have written in favour of deligation. But the fact is, that excess of hemorrhage is not common after division of the peduncle of the polypus; and however great that or any other traumatic hemorrhage from the impregnated uterus may be, it can always be arrested by properly filling the vagina for a few hours with pieces of sponge or other appropriate plug. Dupuytren, though using no plug or other means to prevent bleeding, only met with two cases of severe hemorrhage out of some 200 instances in which he removed uterine polypi by excision; and Lisfranc only observed it twice out of 165 similar operations practised by himself. I believe, however, from what I have myself seen, that the practitioner, in employing excision, must expect considerable hemorrhage in a proportion of cases much greater than this; but at the same time he can, I repeat, always readily *arrest* it when it does occur, with proper plugging; and it is perhaps best—as a general rule—to *prevent* and forestall its occurrence by introducing, for ten or twelve hours after every case of resection, a proper tampon of fine sponge into the vagina. Let me add, that it is well to have each piece of sponge which is used previously transfixed by a strong thread, the end of which should be left out of the vagina, in order to permit of the more easy withdrawal of the plug.

The second objection usually urged against excision is of greater weight, viz., that more or less injury is always liable to occur to the pelvic attachments of the uterus, if it and the polypus are forcibly dragged down by Museux's forceps, or by hooked vulsella, so as to bring the peduncle of the tumour into view before dividing it, a plan followed by most operators; or, on the other hand, if the peduncle is divided while the polypus and uterus are *in situ*, the scissors or

knife employed, are, it is averred, apt to injure and cut the vaginal walls and neighbouring tissues, while worked within the vaginal cavity.



Of late years, I have used a means of excising large uterine polypi, that seems to me to obviate entirely this last class of objections, and by which, as I have been led to believe, the whole operation is much simplified, and rendered both greatly more easy to the practitioner, and more safe to the patient. By the means or instrument in question, the peduncle of the polypus is divided *in situ*, and without any chance of its cutting portion injuring the structures of the vagina or vulva.

The instrument, or polypus knife, to which I refer, is sketched in fig. 1. It is of the form of the usual midwifery hook; with the concavity, however, of the hook not blunt, but turned into a cutting surface by the insertion of a small piece of well-tempered steel blade into it. A transverse section of the curved or cutting portion of the instrument, and of its included knife-blade, is shown in a small figure, No. 2. The entire length of the instrument which I have hitherto employed in my own practice is ten inches—the length of the wooden handle being four inches, and that of the metallic shaft six inches. A shorter instrument might, perhaps, suffice equally well. The cut represents the curved portion, or hook at the extremity, as somewhat wider and larger than in the polyptome made for me in the first instance, and which I have generally employed in practice. Perhaps an increased or a diminished size and width, in the curved hook at the extremity of the instrument would render the operation of division by it more easy, according as the stalk of the polypus was very thick, or comparatively slender. The extreme point of the instrument is blunt and rounded; and the cutting portion or blade is so protected and concealed by it, and by the back wall of the curve, that it can be introduced into and withdrawn from the vagina, without any chance of its edge injuring or dividing the viginal structures themselves. To be always able to discover the direction to which its hooked extremity points after it is introduced into the vagina, the front aspect of the handle is distinguished by having a slight knob or other mark upon it.

In employing this polyptome, the stalk of the polypus is first to be reached by the apex of the first finger of the right hand, introduced along the short anterior or pubic surface of the vagina; the instrument is then pushed by the left hand along this finger as a guide, and passed over or above the peduncle of the tumour, in such a direction that the concavity of the hook will come down upon and embrace this peduncle, as the instrument is pulled again downwards. The next step is to make the blade of the polypus-knife cut through the stalk of the tumour. For this purpose, a little simple traction, with a slight rolling or sawing motion, is all that is generally required. If the tissue of the peduncle is dense and strong, the dividing force of the instrument may be increased by the forefinger of one hand being applied with a tractive power to the blunt extremity of the instrument, while the handle is dragged down and moved in a sawing direction, by the other hand of the operator. Sometimes when the polypus is round and loose, after the curve or hook is applied to its pedicle, the cutting portion of the polyptome will divide this stalk most readily, by merely doubling backwards with the fingers the body of the polypus upon its own stalk, and pulling the knife against the bent peduncle. In such a case, the peduncle is divided as much by pressing it against the knife, as by pulling the knife through the peduncle.

During the last few years, I have removed a very considerable number of uterine polypi of different sizes, and some of them of large dimensions, with this curved polyptome; and I can now speak from somewhat extensive experience of the perfect facility and safety of its employment.

Sometimes soft and slender cellular and canaliculated polypi, usually of an elongated form, are met with in practice, which afford no sufficient resistance for a knife to divide them or their peduncles. In such cases, the peduncle of the polypus is, perhaps, most easily severed, by the careful clip of a pair of blunt-pointed curved scissors. In all other forms of the disease, where the tumour was large and pedunculated, I have of late employed the polyptome. In using it, the patient is placed in bed, in the common position on the left side; and generally the whole operation is accomplished so readily and easily, that she is often not aware that more than a common digital examination has been made. I have several times seen some difficulty attend the removal of the amputated polypus itself from the vagina, after its stalk was divided, in consequence of the great size of the tumour; and in order to effect extraction, I have occasionally been obliged to transfix it with the teeth of a large vulsellum. But hitherto I have met with no special difficulty, in at once and easily dividing the peduncles themselves of the polypi, with this polypus-knife.

The whole operation is simple and safe, expeditious and painless, and approaches, perhaps, more than any other in practice, to the

Asclepedian character of the object of the physician; "*ut tuto, ut celeriter, ut jucunde curet.*"—*Monthly Journal of Medicine, Jan.*, 1855, *p*, 10.

109.—ON FIBROUS TUMOUR OF THE WOMB.

By Dr. Edward Rigby, Senior Physician to the General Lying-in Hospital, and Exam. in Mid. at the University of London.

[The following case is interesting. Mrs. C., aged 33, very young looking, married thirteen years, seven children, the last seventeen months ago.]

Examination Abdominis.—A very hard, globular, moveable tumour is felt just behind the symphysis pubis, extending to the right side, as large as an orange.

Examination per Vaginam.—Os uteri open; uterus enlarged, bulging anteriorly; it is evidently identical with the tumour above the pubes; uterine sound passes $2\frac{1}{2}$ inches, and then passes forwards somewhat further.

℞. Liq. calcii chloridi ℥ss., bis die ex infus. aurant. co.; applic. ung. hydr. ori uteri quâque septimanâ.

℞. Pil. hydr. extr. hyosc. aa. gr. v. tertiis noctibus; mistura ferri et magnesiæ sulphatis omni mane.

May 9th.—Very much better in every respect; has taken the liq. calcii chloridi till within a few weeks, and continues the morning laxative as she requires it. Says that the tumour is decidedly smaller. She walks better, is fatter and stronger; tongue natural. During the last three months the catamenia have been about every three weeks.

The abdominal examination shows that the tumour has certainly not increased, but on the contrary I think that it is smaller; it is softer, and cannot be felt above the symphysis pubis so distinctly as formerly.

In a few weeks let her resume the liq. calcii chloridi.

Not having had the opportunity of making an examination when I last saw her, I cannot speak with such certainty as to the precise amount of diminution which the uterus had undergone, beyond that, as felt with the hand above the symphysis pubis, it was softer and less distinct. The distance to which the uterine sound passed when I first saw her was not more than might be expected in the mother of seven children. The globular shape of the tumour as felt through the abdominal wall, showed that the increase of bulk which the uterus had undergone was more at its sides than as regards its length. The anterior wall was evidently much thickened, and by its pressing on the bladder produced the frequent desire to empty it, as was described among her symptoms. The diminution of the pain and sensation of weight in the pelvis, which arose from the pressure of the enlarged uterus, also justified the inference, that the mass was now smaller.

[In another case, the patient, aged 38, was haggard and feeble with severe neuralgic pain of the sacrum. Dr. Rigby gave the liq. cal.

chlor. in doses of 25 minims twice a-day, and applied the mercurial ointment to the os uteri every week. The medicine was increased weekly till she took 50 minims twice a-day.]

The uterus having descended so very deeply into the pelvic cavity. was, in all probability, the cause of the neuralgic pain which she suffered about the sacral region; in other respects there was a considerable similarity between this and the preceding case; in neither had the disease attained a great size. Both patients were feeble, and broke down in health and strength; and, as their state forbade the use of leeches, I had merely the muriate of lime mixture, and the application of ung. hydr. to the os and cervix uteri to rely upon.

I do not quote these two cases as remarkable for their success; but offer them as pointing out the value of this species of treatment, even where the health and strength have been much reduced. Although the uterine tumour has not so strikingly diminished as in the cases previously reported, yet it was sufficiently so to relieve the pelvic viscera from much injurious pressure, and the patient from constant suffering, which was gradually exhausting her. The uterine tumour had also become softer; the catamenia more regular, and the general health of the patient greatly improved.

It is now two or three years ago that my attention was called to a most interesting paper, by my friend Dr. Oscar Prieger, of Kreuznach, on the treatment of the fibrous tumour of the uterus by the well-known mineral waters of that place. From the practice of Dr. Prieger, sen., as well as his own, he has been enabled to give the results of about seventy such cases, a large proportion of which have been eminently successful. Although he gives the water internally, he appears to produce his most remarkable effects from the use of baths and fomentations, the strength of which has been increased by the addition of the Kreuznach salts evaporated down. In two cases of great severity, (especially one of enormous size and hardness.) and which had set me entirely at defiance, he has produced very favourable changes, with justifiable hopes of still further improvement. For some time I added a solution of muriate of magnesia of the same strength as the liq. calcii chloridi, but not being satisfied with the results, Mr. Hooper has prepared for me an artificial Kreuznach water, so concentrated for easy carriage that a tea-spoonful (℥i.) in a glass of water is the ordinary dose. I commenced this in the case last recorded, and have found it of great value in many cases.

I have also given the bromide of potassium, in five-grain doses, during the last year, having reason to believe that the Kreuznach water owes a considerable part of its activity to the presence of this salt. I have given it alone, or with the combined muriates of lime and magnesia, or have added an entire dose of it to the artificial Kreuznach water. Good results have followed its exhibition; but whether they were owing to its effects or other causes must be decided by further observation.—*Med. Times and Gazette, March* 3, 1855, *p.* 209.

110.—PROLONGED RETENTION OF LIFE BY INFANTS WHO HAVE NOT BREATHED.

By Dr. Maschka.

The long period during which life may, under certain circumstances, be retained by the infant who has never breathed, is a fact full of interest to the physiologist, the medical jurist, and the accoucheur. The experiments of Legallois show that, in the mammalia, the fœtus which has not breathed can resist death from submersion much longer than the fœtus in which respiration has been carried on. Puppies and kittens, immediately after birth, may be kept under water for twenty-eight minutes with impunity; when five days old, they perish after sixteen minutes' submersion; and, when fifteen days old, they die as rapidly as other warm-blooded animals of any age, from deprivation of air. The human still-born fœtus can probably live longer without respiration than any other mammalian fœtus. The following cases are collected in the Gazette Hebdomadaire for December 1st, 1854, from different sources. They are very striking, and very suggestive to the practical accoucheur.

Case. I. A woman, aged 25, who had tried to conceal her pregnancy, was delivered when seated on a tub. The infant, born without any signs of life, was buried in a sand-pit, and, after remaining there for half an hour, was removed, and lived. This case is described by Dr. Weese in 1845, in Badisch. Ann. f. Staatsarz, x, 2.

Case II. In 1850, a young woman was tried by the tribunals of Berlin, who had buried her new-born male infant, believing it to be dead. After an hour, the infant was disinterred, and recalled to life.

Case III. T. P., a servant, aged 23, was delivered in a stable, when leaning against the wall, alone, and in a state of unconsciousness, about half-past four, A.M., on the 16th of October. When she came to herself, she found the infant on the ground, having a spade lying upon it, with its cutting edge turned to the body. She took the infant, which was perfectly cold, believing it to be dead, and, with the placenta attached, wrapped it up in her apron, and buried it in the garden. Suspicions arose that she had been confined: she confessed; and at half-past nine, the infant was dug up from a depth of thirty *centimètres*. It was found lying on its face, with the placenta under the abdomen. Though cold, apparently dead, and pulseless, the cord was tied. For two hours, P., a surgeon, used means to reanimate it, when at last it began to breathe feebly, gradually signs of life became more evident, and it cried. Some slight wounds were observed on its body: wounds in the neck, which did not bleed at first, bled when the infant was restored. It took the breast greedily. On the 17th and 18th, the wounds suppurated; and, on the 19th, it died of convulsions. The physicians entrusted with the judicial autopsy reported that it had been inhumed before it had breathed; and that it had not breathed till after it was exhumed; and that the statement of the mother was

possibly true. She was therefore acquitted of the charge of infanticide, but was found guilty of concealment of pregnancy. This case is reported by Dr. Maschka.

Case IV. A. gave birth, about noon, in a private house, to an infant, who gave no signs of life. For an hour, different unsuccessful attempts were made to animate it. The skin became blue, and gradually warmth left it. It was considered dead, and in three hours it was removed to a cold room. [This occurred in January, and the weather was very cold.] Towards evening, it was closed up in a coffin. During the night, the windows of the room remained open. At 11 A.M. on the following day—that is, twenty-three hours after the birth—Dr. Maschka, being accidentally at the house, was asked to look at the body. It was perfectly cold and blue; the eyes and mouth were shut; the joints and the extremities were flexible: there was neither rigidity nor cadaveric discoloration. Astonished at this latter circumstance, but not really doubting that the death of the infant was real, Dr. Maschka placed the stethoscope upon the region of the heart, when, to his amazement, he distinctly heard the sounds of the heart: they were feeble, and at long intervals. It was impossible to appreciate the impulse of the heart against the walls of the chest, or to perceive any movement in the corresponding intercostal space. Attempts at resuscitation were made, but without any effect. At the autopsy on the following day, cadaveric discolorations and rigidity were present. The lungs were of a deep red, and contained no air: they were heavier than water. There was blood in the left, but none in the right side of the heart. It must be admitted—provided the observation of the reporter be correct—that this infant lived twenty-three hours after birth, and never breathed.

Case 5.—The following case is extracted from the 'Gazette des Tribunaux' of Feb. 20th, 1850:—A woman was tried for attempting infanticide: she had buried her infant, but it was dug up, breathed, and lived. The following are the facts. Marie and Renée lived with their father at Vernantes, in the *arrondissement* of Baugé, (Maine-et-Loire.) On the 16th of May, 1849, Marie was alone in the house with her father. About half-past six, p.m., Renée came home, found Marie in a swoon, and called the neighbours to her assistance. Marie soon regained consciousness. One of the female neighbours, having observed numerous spots of blood, asked Marie if she had been confined. She replied, "No, it is not yet time." Her father, however, having observed the earth disturbed in a place in the garden, asked her to explain it, when she replied, "I have been confined; but, as my child was still born, I buried it in the garden." The child was then disinterred. It was found five *centimètres* (about two inches) below the surface, with its face downwards, and with the placenta attached. Means were used to restore the infant, and it was ere long recalled to life. It was calculated that the infant had been three quarters of an hour under ground. The accused was acquitted.

The elaborate memoir of Dr. Maschka terminates with the following summary:—

1. New-born infants can live without breathing, not merely for half an hour or an hour, but for a very considerably longer period, even in circumstances the most unfavourable.

2. In such cases, there is obviously not only an arrest of blood in the capillaries of the skin, but the vessels of the different organs are either in a state of permanent contraction, or are filled with a column of stagnant blood.

3. The movements of the heart must gradually become very slow. Here, a very important question presents itself. How can there be circulation in the infant without respiration, and therefore without the production of arterial blood? This condition of passive life can only exist when assimilation is at a minimum, and when the oxygen of the maternal blood is consumed very slowly. The maternal blood must in such cases be sufficient to maintain life.

Dr. Maschka's essay is contained in the 'Vierteljahrschrift f. prakt. Heilkunde,' t. iii, 1854. It contains interesting physiological discussions, which will repay perusal.—*Association Med. Journal, Dec.* 8, 1854, *p.* 1104.

111.—ON THE ANÆMIA OF INFANCY.

By Professor Ritter Von Mauthner, Vienna.

The author remarks, that for many years the practice of venesection has been on the decline, and he quotes the words of Professor Richter, of Dresden, "Poverty of blood is, next to tuberculosis and cancer, the increasing evil of our time, which will bring down a gradual deterioration of the race, and therefore merits our most earnest consideration." According to Valentiner, most neuralgic affections are caused by anæmia; and according to Trousseau, chlorosis now prevails in the general pathology of the female. An anæmic mother will produce anæmic children; anæmia may be congenital, or acquired from too rapid development and quick growth. It is difficult to believe in the disease as congenital, the quantity of blood in the infant's tissues being normal. Valentin has shown that the amount of blood in the newly born is proportionately greater than in later life; for a child of 5 to 6 pounds has nearly 2 pounds of blood; while at the age of 30, it barely attains one-fifth of the weight of the body. But, in infancy, as in old age, the watery constituent is more considerable than in middle life.

The cause of this so often congenital anæmia lies in the general corporeal weakness of the mother, whence also it comes that there are so many abortions and early deaths. Want of proper food during pregnancy exerts a potent influence, too commonly at work among the lower orders, oppressed with want and care; and the younger children are more subject to the disease, because the exhausted mother loses

in time the nourishing her children by her own milk, and the father has not the means of procuring a wet nurse. A child born of a mother who has suckled another infant during her pregnancy, generally suffers from poverty of blood. Losses of blood, or profuse mucous discharges, by injuring the mother's health, are to be regarded as prejudicial to healthy fœtus development. An aged or diseased father commonly begets an anæmic child. Congenital syphilis must also be regarded as a cause. The morbid change in the blood being unknown, we must be content with the term, "anæmia syphilitica." It is not always recognised, but is of most momentous importance as regards the rising generation. Should an infant affected with this disease be vaccinated, a peculiar glandular disease is apt to ensue from this second poisoning of the blood. The author proceeds to enumerate the symptoms of congenital syphilis. The child suffers from excoriations about the mouth and anus, &c.; from roseola, pemphigus, eczema, psoriasis, or from a peculiar tenuity, smoothiness, and transparency of the epidermis. They do not possess power to resist external influences; they are long in teething; tuberculosis is apt to ensue in the course of time.

The anæmia of development comes on when growth at any period is very quick. Hence we have the anæmia of dentition, the anæmia of puberty, or chlorosis.

From experiments upon animals, Nasse has shown that animal diet renders the blood more coagulable than vegetable diet, and increases the number of the blood-corpuscles. From sugar and starch-meal there is formed a glutinous lymp plasma, but no corpuscles. A purely vegetable diet, therefore, is not suited for infancy; the more so, from the anatomical fact, the cœcum, that part of the intestinal canal where vegetable digestion goes on, is but imperfectly developed at that period. Anæmic children are very apt to suffer from inflammations. Nature endeavours to excite a re-action from this depressing influence; and statis of the blood commonly ensues in organs unfitted for active circulation; but the exudations tend only yet more to impoverish the blood; and venesections materially increase the evil. One of the evils of infantile anæmia is hemorrhage from the congested and delicate vessels of the large intestine. This occurrence is frequently overlooked, or confounded with other disorders, such as convulsions. The author has verified the fact by dissection.

When the plastic power of the organism, says Canstatt, is exhausted by rapid natural and morbid evolutions, then tuberculosis suddenly forms, and the latent material begins to be deposited upon any excitement in the different viscera. Thus, it is to be feared during the blood-impoverishment of dentition; and it attacks especially children subject to perspirations, and to excited pulsations of the heart.

The practitioner should never forget, in considering the diseases of childhood, that sudden attacks, even death itself, may occur just as easily in children from anæmia as from hyperæmia. Thus, cases of convulsions and other attacks, once regarded as inflammatory, require, in the present day, more careful examination and more accurate diagnosis.—*Jour. für Kind.—Med. Times and Gazette, Nov.* 4, 1854, *p.* 474.

112.—OVARIAN DROPSY TREATED BY INJECTION OF IODINE.

Dr. Simpson mentioned that he had injected a case in his wards a few days ago, from which he had removed 200 ounces of fluid, and that he had tested the urine for iodine, but without success. It was usually present, however, after the injection of a hydrocele. He (Dr. S.) had now injected some 10 or 12 cases of ovarian dropsy; the disease had recurred in a few of the cases, but in the others, the cure had as yet been permanent. The operation was unattended with any bad results. Indeed, the pulse never rose, and little or no uneasiness was expressed, and that only (if present) in drawing the fluid, apparently the result of adhesions. He employed the Edinburgh tincture of iodine, which had also been recommended for its strength by Mr. Martin, in his paper on the cure of hydrocele by injection.—*Monthly Journal of Medicine, Dec.* 1854, *p.* 565.

[In a discussion in the Medical Society of London, Dr. Tyler Smith referred to the cases of Dr. Simpson of Edinburgh, in which this gentleman had injected iodine into ovarian cysts.]

Dr. Simpson had now performed the operation many times, and in one case, at the end of a year and a half, there had been no return of the disease. Dr. T. Smith described a case of ovarian dropsy, under his care, in which he had, on the previous Tuesday, with the assistance of Mr. Nunn, drawn off the fluid, and injected four ounces of the tincture of iodine into the cyst. As regards any danger from the operation, the patient had done remarkably well, quite as well as if only ordinary tapping had been performed. The pulse had not at any time exceeded 100. The smell of iodine had been very powerful in the secretions. Both the urine and saliva had darkened silver very readily, showing that iodine had been absorbed, and probably from the peritoneal surface, as it was difficult to understand that the cyst could absorb, or that its injection could be effected without the entrance of some portion of the tincture of iodine into the peritoneal cavity. The injection of the cyst certainly offered another method of dealing with this formidable affection.

Mr. I. B. Brown remarked that the plan mentioned had been resorted to in Paris in several cases; in only one instance he believed had inflammation, followed by suppuration, supervened, proving fatal

to the patient, showing the little liability of the living cyst to be affected by dangerous inflammatory action. Cases so operated upon should be of the simple kind, such as were generally amenable to treatment by tapping, pressure, &c.

Mr. Hancock had known of one case, in which a French surgeon had resorted to the operation in question, the pain resulting from which was so agonizing, that for fourteen hours the patient had to be held by four men; but the patient was ultimately cured of the disease.—*Lancet, Dec.* 2, 1854, *p.* 459.

[Mr. Windsor's successful case of injection of iodine into the pleural cavity, mentioned at page 105, of this volume is another confirmation of the comparative safety of this practice. If we may inject iodine into the pleural cavity, surely we may proceed to the same practice in an ovarian cyst, and even to other cavities.]

113.—ON THE TREATMENT OF OVARIAN DROPSY BY THE INJECTION OF IODINE.

By I. Baker Brown, Esq.

[Mr. Brown alludes to six modes of treating this disease: 1. Tapping. 2. Tapping, with pressure. 3. Tapping, and injection of iodine. 4. Artificial oviduct. 5. Excision of a portion of the cyst. 6. Extirpation.]

It is to the third plan of treatment I now desire to direct especial attention, viz., tapping and injection of the tincture of iodine into the sac. This mode of treatment is almost new in London practice, not so, however in Paris, nor in Edinburgh. The injection of the tincture of iodine into the tunica vaginalis, for the radical treatment of hydrocele, was prominently brought before the profession, many years since, by Mr. Ranald Martin, and is now the regular practice amongst English surgeons; but the French surgeons have carried their experimental inquiries much further in this direction. Velpeau first suggested its application for effusion into large joints. Bonnet, of Lyons, however, made the first trial, and with perfect success. This success was soon followed by many others in the hands of Velpeau, Robert, and some others. Then Boinet of Paris took up the subject, and applied the injection of iodine into ovarian cysts, and published several successful cases in the 'Gazette Médicale de Paris.' Others followed his example, and amongst the number, Monod, and one of his successful cases fell under my own observation. The French surgeons have also used this injection of iodine for peritoneal dropsy, and it appears, from various discussions and trials, with undoubted success. Our intelligent neighbours have gone further, especially Velpeau, Jobert, Maisonneuve, and Ricord, and have injected the peritoneal sac of hernia, and have obtained by it the radical cure of ruptures; and lately, my colleague in St. Mary's, Mr. Coulson, has tried this plan.

The first real practical application of the injection of tincture of iodine into ovarian cysts, that I am aware of, in this country, is by Professor Simpson, of Edinburgh, who with his usual zeal and industry has pushed his inquiries and experiments very freely; the result of his investigations I shall allude to fully when giving my own practical deductions. I shall now relate a case which I have lately treated in St. Mary's Hospital, and then offer some practical remarks for the consideration of the Fellows.

Case.—Mary Baker, kindly sent to me by Mr. Coulson, was admitted into Boynton Ward on December 9th, 1854. She stated that she had had two children, and one miscarriage. She was twenty-seven at the birth of the first, and twenty-nine at the birth of the second child; from the latter period she has noticed herself getting much larger round the waist, and has been much troubled by flatulency; has always been regular in menstruation all through her illness; she suffers no pain, but considerable inconvenience from leucorrhœa.

Examination. Dec. 12, 1854.—I discovered a well-marked ovarian cyst, apparently unilocular, and fluctuation distinct. The measurement around the abdomen was 29 inches below, and 28 above the umbilicus. I placed her under medical treatmenr, with a view to improve her general health, which was much impaired by the secretion of the fluid into the cystic cavity, and on

The 20th, I proceeded to empty the cyst; first placing the patient on the table in the horizontal position, I introduced a large trocar through the semilunar line, and evacuated twenty pints of a turbid thin fluid, which was found to be strongly albuminous, becoming almost solidified by the joint application of heat and nitric acid; it also contained abundant crystals of cholesterine. I then introduced a long flexible catheter, and through it injected five ounces of the tincture of iodine, (Edinburgh Pharmacopœia,) which is about double the strength of the London Pharmacopœia—the pain experienced was very trifling, described by the patient merely as "smarting:" the wound having been closed by strapping, I applied appropriate pads, and one of my many-tailed bandages; the patient was then placed in bed, and two grains of opium given, and four ounces of port wine ordered for the next twenty-four hours.

In my case-book, I find Dr. Vernon, the resident medical officer in charge of the patients in the ward for diseases of women, has made the following notes:—

10 p.m. Pulse 76—expresses herself as being very comfortable, having neither pain nor tenderness in the abdomen; complains only of a bad taste in the mouth—her breath smells of iodine.

21st, 10 a.m. Feels comfortable,—has slept only two hours during the night,—has voided three and a-half pints of urine since the operation, the fluid injected having amounted only to two pints; says she has a nasty taste in her mouth, somewhat like sea-weed.

1 p.m. Complains of her cough being troublesome, and of pain in

the abdomen on both sides when she coughs—the pulse has mounted up to 110—discontinue the wine and take a tincture for the cough—continue the opium pill every six hours.

22nd. Amount of fluid taken, 3 pints; voided, 3 pints; bowels relieved this day. Pulse 90.

23rd. Amount of fluid taken, 1½ pints; voided, 1 pint. Pulse 76. No pain or tenderness in abdomen.

24th. Amount of fluid taken, 1½ pints; voided 1 pint. Pulse 74. Skin cool; and she complains only of the nasty taste in her mouth.

25th. Has taken 2 pints of fluid, and voided 1 pint.

26th, 27th, 28th, 29th. The same.

From this date to the 15th of January she took a diuretic mixture, and the effect on the secretions was, that the amount of fluid voided corresponded with the amount taken.

It was now apparent that there was some refilling of the cyst, but it proceeded very slowly, her appetite was good, she slept well, and suffered no pain.

20th Jan. There is apparently about two quarts of fluid in the cyst, but it does not appear to increase, and the patient is decidedly better in health. I now ordered her to wear one of my ovarian bandages to keep up gentle pressure over the whole abdomen, so as to give support to the parietes, and also to arrest the refilling of the cyst. In a few days she left the hospital in good health, considering herself much improved, and exhibiting no external signs of disease. I have lately seen and carefully examined her, and can find no increase of fluid, but great improvement in her general health, and she has resumed her laborious duties of laundress, and states she feels quite well and is in good spirits.

Before offering some practical observations I am desirous of drawing the attention of the society to the important fact of the iodine having been taken up speedily into the system, as evinced by the smell of the breath, and the taste in the mouth. I was anxious to ascertain whether it was to be found in the urine, and therefore requested our dispenser, Mr. Copney, to examine some for me on the day following the injection. He says:—

"The urine gave unmistakable evidence of the presence of iodine by the following tests:—

"1. The formation of the blue iodide of starch of considerable intensity.

"2. By its forming with the salts of lead and mercury the characteristic iodides of those bodies.

"3. By the production of hydriodic acid.

"4. By the elimination of the iodine itself."

Three days later, I examined the sweat of the patient, but could then detect no iodine, although it is probable that, if I had made the examination sooner, I should have found evidence of its elimination.

Practical Remarks.—The first question in a practical point of view is, Has the treatment in this case been successful? The answer is,

that it has been only partially so at present; for, although it has not prevented some return of the fluid, still it has evidently controlled the refilling of the cyst, and the general health has much improved; further time is required to test the extent of benefit. My own impression is, that in such a case the injection of iodine is not likely to effect a complete cure, yet that it will arrest the refilling. I believe that those cases which are radically cured by this plan are the more simple form of unilocular ovarian dropsy,—*i.e.*, where the cyst is thin, and its contents non-albuminous, or slightly so. The dropsy of the broad ligament is especially favourable for this plan; but then, so it is for simple tapping and pressure, and the latter plan is a less hazardous one than the former. I apprehend we cannot expect to solve the whole question until we know more of the pathological structure of the different cysts, in each form of the disease. I am investigating this question by aid of the microscope, and I hope to arrive at some certain facts as to the peculiar condition of the secreting surfaces, and the peculiar fluid from those surfaces; so that, by drawing off a small portion of fluid, we may be so certain of our diagnosis, as to recommend with greater confidence the adoption of any mode of treatment best fitted for that especial case. It will be seen at once that this will prove of the greatest practical importance; and I do not at all despair of being able to form certain general rules from the results of these inquiries; and I shall feel particularly indebted to any Fellow of the Society who will favour me with a post-mortem specimen of this disease. Much has already been done during the last twenty years, and I trust to see much more done in future years.

In the 'Monthly Journal of Medical Science' for May 1854, is the following opinion of Dr. Simpson:—

"Dr. Simpson has, within the last year, injected into dropsical ovarian cysts, subsequent to tapping, the tincture of iodine of the 'Edinburgh Pharmacopœia,' undiluted, in seven or eight cases. He has usually thrown into the cyst two or three ounces of the tincture. In some cases, he has allowed a portion of the injected fluid to escape; in others, has retained the whole of it. From these cases he drew the following conclusions:

"1. In none of the cases of ovarian dropsy treated with iodine injections after tapping, has he yet seen any considerable amount of local pain follow the injection, with one exception; in most instances no pain at all is felt; and in none has constitutional irritation or fever ensued. In the one exceptional case considerable local irritation followed, and the pulse rose to 110; but the same phenomena occurred in the same patient after previous tappings, without iodine being used.

"2. While the practice seems so far perfectly safe in itself, it has by no means proved successful, as in hydrocele, in preventing a re-accumulation of the dropsical fluid; for in several instances, the effusion into the sac seems to have gone on as rapidly as after a simple tapping without iodine injection.

"3. But, in two or three of the cases, the iodine injection appears to have quite arrested, for the time being, the progress of the disease, and to have produced obliteration of the tapped cyst, as there is no sign whatever of any reaccumulation, though several months have now elapsed since the date of the operation.

"Lastly. Accumulated experience will be required, to point out more precisely the special varieties of ovarian dropsy most likely to benefit from iodine injections, the proper times of operating, the quantities of the tincture to be injected, and other correlative points. Perhaps the want of success, in some cases, has arisen from an insufficient quantity of iodine being used, and from the whole interior of the cyst not being touched by it. The greatest advantage would of course be expected from it in the rare form of unilocular cysts. In the common compound cyst, the largest or most preponderating cyst is usually alone opened in paracentesis; and though it were obliterated, it could not necessarily prevent some of the other smaller cysts from afterwards enlarging and developing into the usual aggravated form of the disease."

I have preferred giving the whole abstract, so that Dr. Simpson's opinion might be fully understood. It will be seen that he used only half the quantity of the tincture that I have used; and I intend, in the next case, using 10 or 12 ounces, so as to insure, if possible, the complete covering of all the cystic surface.——*Med. Times and Gazette, April* 14, 1855, *p.* 359.

114.—TREATMENT OF HYDATID TUMOURS IN THE ABDOMEN.

[The Editor of the Medical Times and Gazette appends the following general remarks to an interesting series of reports of abdominal tumours.]

Operative Measures.—It has been attempted to show that operative interference should be reserved for cases in which urgent symptoms impend. An hydatid tumour in the abdomen may cause urgent symptoms, *first*, by acquiring a very large size, and distending the cavity; *secondly*, by interfering with the function of the viscera adjoining it, as the liver, kidney, &c.; *thirdly*, by occluding, by pressure on the intestines, ureters, &c., the passage of the excretions; *fourthly*, by exciting surrounding inflammation, and threatening to rupture. In any one of these conditions, surgical assistance may come to be demanded, but the measure required will differ considerably in each. Thus, when the cyst is so large as to have distended the abdomen, it will generally have acquired adhesions, so that its puncture will be comparatively without risk. The same will be probable if it have excited surrounding inflammation, under which condition also it may become necessary to make a permanent opening for the escape of pus, when otherwise a

trocar puncture would have sufficed. The cases, however, in which urgency is caused by interference with the viscera, are by no means secure from the risk that the tumour may be quite unconnected with the parietes; and in the treatment of these the greatest judgment will be called for. If the tumour be in the lower half of the belly, examination *per rectum*, and *per vaginam* if the patient be a woman, should never be omitted, as it might chance that a puncture would be best made into one or other of these canals. A case has been already mentioned, in which a rectal examination would, probably, have saved the life of the patient. Supposing, however, that it seems necessary to make an external opening, and that there is reason to believe that the cyst has no adhesions to the parietes, the surgeon has then the choice of two methods—the use of a very fine trocar, or the application of an escharotic. The latter means was employed very successfully by Mr. Le Gros Clarke, in a case under his care, and the former may be confidently stated to be very safe. The selection will, therefore, depend chiefly upon whether it be wished to make a permanent opening or not. And this brings us to the consideration of the important question already mooted, as to whether it is absolutely necessary to obtain the evacuation of the contents of the cyst, or whether to kill it, and then trust to their absorption, will suffice. Hitherto the plan of simply destroying the life of the cyst, has, we believe, not been tried In Mr. Le Gros Clarke's case, however, it probably occurred, and the cure resulted without the removal of the smaller cysts, as had been intended. This accidental result is very instructive. Might it not be worth while, in future cases, to take means to secure the death of the parasite, such, for instance, as the injection into it of a solution of salt, or of iodide of potassium? The evacuation plan necessarily involves long-continued and profuse suppuration, which, supposing the cyst large, may become very dangerous. At any rate, whichever plan be selected, there can be no doubt as to the propriety of conducting the first steps of the treatment on the plan exemplified in the case which did so well under the care of Mr. Cock, namely, of reducing the size of the tumour by repeated punctures with a very small trocar. Hydatid cysts re-secrete very slowly, and by such means great diminution in size may be effected, almost without risk. The trocar should be an exploring one, and of very small bore.

We must not leave this part of the subject without venturing one further remark, to the effect that a patient, suffering from urgent symptoms, however produced, in consequence of an hydatid tumour in the abdomen, should never be allowed to die without the attempt to relieve by operation being made. The recorded cases have been quite sufficiently successful to indicate the propriety of making such attempt, even under very unhopeful circumstances.—*Med. Times and Gazette, March* 17, 1855, *p.* 260.

MISCELLANEOUS SUBJECTS.

115.—ON THE PROPERTIES OF THE ORDEAL-BEAN OF OLD CALABAR.

By Dr. Robert Christison, Professor of Materia Medica in the University of Edinburgh.

[Dr. Christison gives us a long and interesting account of this poisonous bean and the use made of it by the natives of old Calabar. The effects which he has himself observed are detailed at length in the Edinburgh Monthly Journal for March, and are accompanied by the following excellent remarks, on the effects of some other poisons on the system.]

When a poison impresses powerfully both the circulation and some function or functions of the nervous system, it is a matter of great nicety to eliminate the true phenomena, especially by observation upon the lower animals alone.

We know that some poisons, such as strychnia, and the various seeds and barks which contain it, cause, by direct irritation of the spinal chord, violent tetanic spasms of the voluntary and respiratory muscles, without impairing sensation, or enfeebling the heart, or clouding the mental faculties; and thus they occasion death by convulsive arrestment of respiration. Others, such as the urari poison, and conia, or hemlock from which it is derived, cause, by direct exhaustive action of the spinal chord, the opposite state of paralysis of the voluntary and respiratory muscles, but still without influencing the heart, or sensation, or the mental powers; and so death arises in their instance, from arrestment of respiration, by simple paralysis of the muscles which maintain it. Others again, such as atropia, or belladonna, the plant which yields it, principally assail the functions of the brain, at first combining stimulus of some with exhaustion of others in the most singular and often grotesque concatenation, but inducing at last a state of profound coma, and as the result of this a universal muscular paralysis; and thus death ensues, equally as before, from the arrestment of the breathing, not however by direct action on the origin of the nerves which govern the muscles of respiration, but indirectly, through an influence on the cerebral

functions, exactly as in ordinary apoplexy. We can likewise conceive a poison to possess only a simple and direct action upon the heart, producing exhaustion of its irritability, paralysis, and consequently death, by arrestment of the circulation; but no such poison is yet known.

These are all instances of simple action on a single vital function. But many poisons exert a more composite action. Some, such as nicotina, and its source, tobacco, produce paralysis of the heart, and also a narcotic action on the brain. Others, such as foxglove, and in all probability its active proximate principle, digitaline, not only possess this double action on the heart and brain, but likewise powerfully irritate the kidneys. Others, such as hydrocyanic acid and picrotoxa, the active constituent of cocculus-indicus, exhaust the functions of the brain, so as to induce coma, and at the same time irritate the spinal chord, so as to excite convulsions; and thus, here again we have death produced by arrestment of the breathing, indirectly through the brain, but concurrently with direct spinal irritation. In others, such as aconitina, and its source, monkshood, there is a singular combination of exhaustion of the heart's irritability, and of common sensation, but without any influence on the voluntary muscles, or on the mental faculties; and death arises by arrestment of the circulation.

It is easy to see,—on considering attentively what must be the manifestations of these various actions, both simple and compound, but especially the latter,—that exreme difficulty will often occur in seizing and rightly comprehending the facts, above all when the succession of phenomena is swift, and when the subject of observation is one of the lower animals, which cannot adequately express by external signs the varying influence of agents on sensation and the other cerebral functions.

Hence it arises that many erroneous conclusions have been come to regarding the action of our most potent and interesting poisons. Take for example hemlock. This formidable poison was long supposed to cause death by coma, that is, a narcotic action on the brain. But I have shown in a paper read before this Society in 1836, that the mode of death is really by paralysis of the muscles and arrestment of respiration, through an exhaustive influence on the spinal chord. And it is easy to see where the source of error lay. For, when the muscles are paralysed, sensation and the mental faculties will seem to a common observer to be paralysed also; because the animal mechanism for producing expression is at rest.—It appears that many persons think it an easy task to investigate experimentally the physiology of poisoning. But they are assuredly mistaken. A long apprenticeship must be passed before any one can observe with accuracy the phenomena of the action of poisons.—*Monthly Journal of Medicine, March* 1855, *p.* 198.

116.—*Note on the Therapeutic Action of the Salts of Cerium.* By Prof. Simpson, Edinburgh.—The salts of few metals were at present in our pharmacopœia, and Dr. S. saw no reason why many more might not be added to their number. He had recently drawn the attention of the profession to the salts of nickel, which, as far as his observation went, presented much similarity in action to those of iron and quinine, and seemed to be of use in cases of sick headache. He now proposed to read to the Society some very imperfect observations on the therapeutic action of some other metals: and first, as to cerium, which, given in the form of nitrate, and in one grain doses twice or thrice a-day, appeared to act as a sedative tonic of considerable value, strongly resembling bismuth, and the salts of silver. He had employed it in the first instance in cases of general chronic intestinal eruption—a peculiar and intractable form of disease, for which arsenic and nitrate of silver were generally prescribed, and where these remedies had failed, cerium had been tried with marked advantage. In irritable dyspepsia, with gastrodynia and pyrosis, and in chronic vomiting, its exhibition was attended with satisfactory results; and in the vomiting which occurs during pregnancy prompt relief was afforded. It was a good tonic, and a useful substitute for the salts of silver, bismuth, and hydrocyanic acid. Dr. S. had not employed it much in convulsive diseases, as chorea and epilepsy, in which nitrate of silver was used, but the exhibition of the salts of cerium was certainly attended with this advantage, that it could be persevered in without any fear of discolouration of the skin. As far as his experiments with cadmium went, it bore much resemblance to the preparations of antimony, and excited diaphoresis and vomiting. Tellurium, besides its expense, was precluded from being used in practice by its disagreeable effects. Dr. S. mentioned the particulars of a case where a dose had been inadvertently given to a student of divinity, and had been followed by the evolution of such a persistent odour, that for the remainder of the session the patient had to sit apart from his fellow-students.—*Monthly Journal of Medicine, Dec.* 1854, *p.* 564.

117.—*Anaphrodisiac Properties of Bromide of Potassium.*—Thielmann recommends this remedy as an excellent anaphrodisiac in satyriasis, in the frequent and painful erections during gonorrhœa, in spermatorrhœa, and in nymphomania. He administers it in doses of from 2 to 3 grains every two or three hours; and, at the same time, enjoins a mixed vegetable and milk diet, and forbids all acid substances.—*Med. Ziet. Russi.—Dub. Hosp. Gazette, Feb.* 15, 1855, *p.* 32.

118.—*Treatment of Simple Incised Wounds.* By Prof. Syme, Edinburgh.—[If we close a wound too soon it often refuses to unite, on account of the oozing of the blood upon the surfaces. It is often the best plan therefore, to defer dressing the wound for a time. This,

however, occupies valuable time, and Mr. Syme therefore adopts the following plan. He says,]

After no long time, however, I found that it was unnecessary to keep the patient all this time in a state of suspense, when he ought to be at rest, and then subject him a second time to disturbance; for, still following out the same principle, I saw that, by placing pads of lint upon the sides of the wound, but leaving their edges free, the wound might be permanently dressed at the time of the operation so as to attain the same object—the pressure of the pads, like that of the pin in hare-lip, preventing the accumulation of blood in the deep parts, while sufficient interval was left between the stitches to allow its escape between the edges of the wound in the skin, which were preserved free from pressure. Accordingly from that time to the present—that is to say, for thirty years—I have followed this method, and I am happy to say, union by the first intention has been always obtained, unless there was some unusual circumstance to interfere with it. The dressing is not disturbed for three or four days after the operation, when the lymph between the raw surfaces is already of considerable firmness; and the same dressing is usually continued afterwards, as it requires to be changed only every other day, and involves less disturbance than any other method. In illustration of the same principle, I may remark, that, when the cavity of the wound is peculiarly irregular, or when the situation is such as to make it difficult to apply pads of lint effectually, I find a large sponge a very useful dressing. Thus, after the removal of tumours of the neck above the clavicle, or at the angle of the jaw, I have by this means obtained complete union by first intention.—*Lancet, March* 31, 1855, *p.* 334.

119.—ON POLYPUS OF THE NOSE.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[In a clinical lecture on this subject, Mr. Syme remarks, that the nose of the patient was small and the nostrils narrow, and that the growth evidently extended into the throat, which could be ascertained by the finger.]

In considering the means that should be employed for the removal of this polypus, I beg to remind you that non-malignant growths are very limited in regard to the seat of their origin from within the nose, as they never proceed from the floor, septum, or external wall, and always do so from the roof, or that part which is formed by the turbinated plates of the ethmoid bone. Further, I must remind you that each nostril affords very little space in its transverse direction, and that the narrow slit which constitutes it is divided longitudinally into two nearly equal portions by the inferior spongy bone. Now the mucous polypus, always originating from the small extent of surface that has been thus defined, whether it does so singly or, as more frequently

happens, in groups, descends so as to occupy first the higher and then the lower channel of the nose, but cannot attain any great size so long as it is limited to these situations; and not possessing sufficient vigour of expansion to enlarge the osseous space, in order to attain any considerable bulk, must therefore proceed backwards towards the pharynx. Keeping these things in mind, you will at once perceive that the plan of removing polypus by means of a thread or wire, whether simply put up the nose, or passed through it into the throat, cannot possibly extirpate the growth, or do more than remove a portion of its substance. You will also require no arguments to prove the inutility and inexpediency of trying to accomplish the object in view by scissors, knives, or caustic; so that our choice is thus limited to evulsion by forceps.

This operation is generally regarded as very unsatisfactory, from the relief which it affords being seldom complete, and still less frequently permanent. But these imperfect results should in a great measure be ascribed to the faulty nature of the procedure usually employed, which is, aiming at the body of the growth to be removed instead of its narrow root, so as to detach merely the most projecting portions, instead of extirpating the whole mass entire. It is therefore no wonder that the operator fails to relieve the patient, or finds many "sittings," as he calls them, necessary, and nibbles away, in vain attempts to effect a radical cure. The forceps that were formerly employed on such occasions are indeed of themselves sufficient to show the utter hopelessness of succeeding by their means, since the blades are so large that they could not be insinuated into the narrow channel where the root of the growth is attached. You should select for the purpose an instrument such as that in my hand, which is of the smallest possible size, consistent with the requisite degree of strength; introduce it gently along the upper passage of the nose, with the blades separated so far as the walls of the cavity permit, until its further progress is arrested by the neck of the polypus; then firmly compress the handles, and, by a combination of traction with torsion in one uniform direction, break through the connecting textures included. If the growth does not follow the instrument, it must be introduced again and again in the same way until the nostril is completely cleared, when a piece of lint may be introduced to prevent further bleeding.

[Mr. Syme then proceeded in the way that has been described, and after several introductions of the forceps, succeeded in removing a large round polypus quite entire, immediately after which the patient breathed with perfect freedom through both nostrils.]

It seems not unworthy of notice that in this case there are some points of resemblance to that remarkable tumour named the fibrous polypus, of which the characters are unity of growth, ligamentous toughness of consistence, extreme firmness of attachment, and disposition to bleed. Several cases have occurred here in which the patients had nearly died of hemorrhage, and all the strength that I could exert was hardly sufficient to effect extraction, while blood streamed in tor-

rents from the nose and mouth. But as this disease is not malignant, the operation, however formidable, proves satisfactory in its result. Now, you may have remarked, that the tumour which has just been removed was solitary, and confined to one nostril, instead of occurring in both, as the mucous polypus almost always does; also that its connexions were very firm, and that the bleeding was considerable. It would seem as if there may be conditions intermediate between the two sorts of growth, in which the respective characters more or less closely approach those of each other. For instance, some years ago, a nobleman returned from public service in one of the colonies, on account of complete obstruction to breathing through his nose, with a constant discharge of watery fluid from it, and occasionally alarming hemorrhage, of which the amount was estimated not by ounces but by pints. He remained in London for two months under the care of several practitioners, who regarded the disease as incurable; and finding himself thus given up as hopeless, he applied to me, bringing with him a letter from the late Mr. Bransby Cooper, which stated that the result of repeated consultations with Sir B. Brodie, Mr. Travers, Mr Cæsar Hawkins, and himself, was a decision against interfering with the disease, which they seemed to regard as a tumour of the throat. Seeing no sign of malignant disposition in the aspect of the patient, who was about thirty-four years of age, and recognising in the tumour the characters which were familiar to me as those of a nasal growth stretching backwards into the pharynx, I at once undertook to remove the source of complaint. The patient and his friends at a distance having given their consent, I introduced the forceps, and at the very first pull extracted the whole growth, with immediate, complete, and permanent relief. Four years have now elapsed, and there has not been the slightest symptom of relapse to interfere with the enjoyment of perfect health, or the vigorous exertion of an active life.—*Lancet, Jan.* 6, 1855, *p.* 2.

120.—ON A NEW METHOD OF TREATING BRONCHOCELE.

By William Turner, Esq., Kensington.

[Mr. Turner first relates the following interesting cases, and from them proceeds to detail his new method of treating bronchocele.]

Being called in 1845 to a Mrs. H., of Holland-street, Kensington, an aged female, suffering from phlegmonous erysipelas, which had extended from a wound in the hand, and now involved the whole arm, neck, and side face, (the neck being enormously swollen owing to a very large general bronchocele as well as the erysipelas). This large bronchocele had existed forty years, and on a former occasion I got to examine, (although the doctors had told her it would be the death of her, if interfered with,) and found a large fluctuating cyst imbedded

in the mass of enlarged thyroid. This foreknowledge emboldened me now to make a free incision into the mass, with the hope of relieving the present distressing symptoms impending suffocation. There was most urgent dyspnœa, purely, I judged, from the great tension about the throat; countenance livid, the tongue protruded, and every evidence of such trouble to the respiration and circulation as must quickly terminate life if unrelieved. A free incision into the large cyst discharged about a pint of serous, fetid fluid, at the same time giving immediate relief to the most urgent symptoms. The cut edges of a thick yellowish cyst became evident about an inch from the surface of the skin. The opening continued to discharge, and, by aid of a female catheter, I withdrew from time to time the liquid fetid contents which accumulated in the lower part of the cyst, until by degrees, and at length after six weeks, the whole cyst came away, and the opening in the neck healed; and, to my surprise, after many months, the enlarged thyroid body gradually got less, so that after two years she had lost all her bronchocele.

In 1849, Mrs. R., of High-street, Kensington, appeared to be suffering much pain and distress in breathing from a suppurating cyst, developed in a large but unilateral bronchocele. This I perforated with a trocar and canula, and discharged fetid matter from it. The discharge in this case continued for months; no cyst came away; but yet the thyroid enlargement has so much decreased as to be scarcely visible. Also she can now do needlework. Before the operation, the tumour prevented her looking down on her work.

From these and other cases it was imagined, if inflammatory action could be induced and maintained for a time, it might, as one observes sometimes in the testicle, and as these cases in the thyroid appeared to demonstrate, be followed by adhesive and constructive inflammation from effusion and infiltration ultimately giving rise to absorption from obliteration of nutrient vessels.

The first case operated on with this intention was in 1852. Mrs. C., of Vale-place, Hammersmith, aged 43, of spare, delicate habits, complaining much of the trouble and inconvenience to her breathing produced by a unilateral enlargement of the thyroid body. This was about the size of a large lemon, and had resisted all the ordinary means employed without any good result; therefore, selecting the most prominent part of the tumour within the inner edge of the sterno-cleido mastoid, I applied the potassa fusa over a space the size of a florin. On the separation of the slough, produced after twelve days, the potass was again applied, and repeated at intervals, until the thyroid was exposed, by the gradual destruction of the superincumbent tissues. Next, a blunt-pointed grooved director was passed freely into the substance of the enlargement in various directions. This gave rise, after some days, to considerable constitutional irritation and feverish disturbance, with heat, pain, and some slight swelling in the thyroid, followed by a copious watery discharge from the open-

ing; the patient imagined a pint and a-half came off daily for two weeks, and then gradually subsided, as did also the swelling and constitutional irritation. The patient's present condition, free from any enlargement and with the slight disfigurement produced by the eschar, is most satisfactory.

Mrs. M., of Warwick-square, Kensington, aged 36, a nervous, delicate lady, of thin, spare habit, mother of a large family, requested relief from a unilateral enlargement of the thyroid, giving rise to difficulty and a noise in respiration on any active exertion; complained of starting in alarm from sleep, imagining some one was strangling her; could not read aloud, as was her former custom; for many months had been subjected to various plans of treatment, without relief.

The same mode of treatment was adopted in this case as that last detailed, with the caustic potash. About the period when the gland was exposed, the patient's health became rather suddenly disturbed from uterine derangement, followed by general nervous excitement and irritation. I was obliged to desist from further measures as regarded the neck. The patient slowly recovered from this low nervous fever (*secundum* Sir James Clark, who visited her when in this state.) A portion of the thyroid, the size of a walnut, blackened and spongy, came away after a time, without hemorrhage. The opening gradually healed, and this lady is now in robust health, and although relieved of the urgent symptoms complained of, the site of the former swelling, as well as the eschar, is now in a depression. Still, the posterior portion of the gland behind the sterno-cleido mastoid has much increased since the operation, now nearly two years; yet, as I had not the opportunity to pass the probe through and among this portion, I do not consider it as having been subjected to the proposed plan. The perfect freedom from hemorrhage, by the employment of the method proposed, has much surprised me. The caustic potash seems primarily to seal up the vessels by chemical action, and, secondarily, by inducing inflammatory action on the surrounding structures, the blood-vessels are sealed up prior to the separation of the slough produced at each step in the progress. I imagine vessels of some considerable size are thus quietly cut through without bleeding.

Although the thyroid body in its natural state is not contained in any distinct capsule or membrane, yet, by its enlargement, it appears to condense the cellular tissue and fascia about it into a dense membranous covering, which would well resist the passage of a blunt probe or grooved director, unless inconsiderate force was employed; and, as all the larger vessels are chiefly confined to the upper and lower margins of the lateral lobes, one does not risk them in perforating the most prominent portion of the tumour, which always presents itself, either in front or behind the sterno-cleido mastoid; in the former case, covered beneath the cervical fascia by the expanded

fibres of the sterno-thyroid and hyoid; in the latter by fascia latissimus colli and omohyodei.

Although very many kinds of treatment have been directed to the relief of bronchocele, not a few have had the great disadvantage of being more desperate and dangerous than the disease; and some may imagine the one now proposed of the same class; yet, from my experience, I do not find it so, and consider it worthy of a more extended trial by those of my profession who can command beds in hospitals, and have a better opportunity of keeping close watch on such cases than one engaged in general practice can do. With this impression, I have been induced to bring these observations before them, and must crave all indulgence for this hastily-written paper.—*Med. Times and Gazette, Jan.* 20, 1855, *p.* 56.

121.—*Note on the Performance of Tracheotomy.*—A few words may perhaps be permitted us concerning the operation of tracheotomy, more especially as the point to be adverted to is mentioned in few if any books, and, though of manifest advantage, is rarely acted on. It consists in the introduction of a hook into one side of the trachea *before* opening it. By this means, the trachea is held steady, and the making of the incision much facilitated; and, what is of yet more importance, the wound made is afterwards easily kept open and prominent for the introduction of the canula. The many expedients which have, from time to time, been devised for the performance of tracheotomy, are evidences of the difficulties which attend it; but none have, as yet, been able to supersede the old method by the scalpel. We mentioned at page 267, in the 'Med. Times and Gazette' for Sept. 10, 1853, a case in which great disappointment attended an attempt to employ the trocar; and at page 418, for Oct. 21, 1854, is the narrative of a lamentable and fatal accident with Dr. Marshall Hall's tracheotome. The use of a hook, in the manner suggested, obviates, to a very great extent, the difficulties attending the scalpel method, and constitutes it by far the safest and most preferable. A common tenaculum will serve the purpose very well; but by some instrument-makers a hook, especially adapted to it, is sold with the tracheotomy case. A case in which the operation was so performed will be found at page 136, in the 'Med. Times and Gazette' for Aug. 5, 1854. We have since witnessed an operation, during which, for want of the hook, considerable difficulties were encountered.—*Med. Times and Gazette, Dec.* 9, 1854, *p.* 593.

122.—ON THE BRAIN.

By Joseph Swan, Esq.

It seems that all the fibres of the human brain must undergo some change for bringing them to a proper condition for activity and steadiness, and this is the reason why all manual arts should be begun

early, and even intellectual pursuits, as they are with so much difficulty allowed to come to perfection in mature age. Every fresh impulse requires one or more fresh fibres of the voluntary tract for its perfect reception, and thus so large a brain is necessary for the almost unbounded extent of the powers of the intellect of man; therefore, however hard he may work, and however much knowledge he may acquire, there is always room left for further stocks of information. He can occupy as much of his brain, however, as he pleases, and according to the degree of his industry, or idleness, will be his intellectual progress, so that the extent of his attainments rests with himself. By the necessity for such use and proceedings, he becomes possessed of a deliberative power of acting, and of using his muscles, and thus gains the form and character of his hand-writing, which he retains to the end of life, whether it be good or bad; the one has been perfected by care and attention, the other by slothfulness and negligence, unless bad instruction or some disorder has prevented the proper development of his powers. In the same way artists form and retain good or bad methods of drawing, and artificers excel or not, in their several branches.

A large brain is generally considered to be favourable to the extension of the intellectual faculties, but a small brain has also been found capable of promoting high attainments. The difference may depend on the quality of the fibres; but the small brain, without any difference of composition, may, through assiduous study, have been sufficient for allowing a great extent of knowledge, and as much as is usually reached by a larger brain, which is seldom exercised to its utmost limits. It is difficult to determine how small a brain is capable of as much intellectual power as will not be considered low enough for constituting an imbecile person or an idiot, until attempts at instruction have been prosecuted extensively enough for determining its capability of ministering to intellectual functions.

In early youth, the brain is not only the most susceptible of impression, but the most capable of retaining it. As life advances, the brain varies in respect of its powers, and loses much of its susceptibility of imprinting letters and words so that their memory may be permanent. It is then maturity of thought and judgment exercises the greatest power, and allow the mind to receive the highest qualities of information. If the two seasons are reversed, the advantages of either are gained with great difficulty and uncertainty. It is most probable that unless a person is gradually educated from his youth, he will not attain to excellence either in learning or the arts; and hence the great difficulty in changing his position and leaving off an accustomed business, which depended on manual dexterity.—*Monthly Journal of Med., March* 1855, *p.* 238.

123.—APPLICATION OF ELECTRO-CHEMISTRY TO THERAPEUTICS.

Chemistry is about to save from death, or a premature old age, those artizans whom the exercise of a cruel profession condemns to breathe metallic dust or vapours, who poison themselves daily for the sake of living, and acquire so many dreadful infirmities in the silvering of looking-glasses, the preparation of white lead, or working in the mines. Science comes to the help of the victims of industry or pleasure, and extracts from their bodies, atom by atom, the devastating metal that had fastened on their tissues, and weighed on the springs of life. These hopes are drawn from a memoir presented to the Academy of Sciences by M. Dumas, and the authors of which—MM. A. Poey, of the Havana, and Maurice Vergnés—will hold a distinguished rank among the benefactors of mankind, if experience confirms their assertions.

The invention consists in the application of electro-chemistry to the cure of the diseases we have mentioned; and surely, of all its marvellous uses, this would be the most admirable.

M. Poey takes an unfortunate patient, corroded by lead, mercury, gold, silver, or any other metal, and places him in a metallic bathing-tub, isolated from the ground. The man sits down, his legs horizontally stretched out on a wooden bench, isolated from the tub, which is filled with water up to his neck. The water is slightly acidulated, to increase its conductibility; and the acid varies according to the cases. Nitric or hydrochloric acid is used for the extraction of mercury, silver, or gold; sulphuric acid for that of lead. This done, the negative pole of a pile is brought into contact with the sides of the bathing-tub, and the positive pole placed in the hands of the patient.

The work of purification is now in full activity; the electrical current precipitates itself through the body of the sufferer, penetrates into the depth of his bones, pursues in all the tissues every particle of metal, seizes it, restores its primitive form, and, chasing it out of the organism, deposits it on the sides of the tub, where it becomes apparent to the naked eye.

In this great discovery, chance or accident has played a part. One of the inventors—M. Maurice Vergnès—occupied himself with galvanic gilding and silvering. His hands being in continual contact with solutions of nitrate and cyanuret of gold and silver, got covered with ulcers in consequence of the introduction of metallic particles. One day he plunged the diseased organs into the electro-chemical bath, at the positive pole of the pile; and, after a quarter of an hour, to the great surprise of the beholders, a small plate of metal brought into contact with the negative pole covered itself with a thin coating of gold and silver, extracted from the hands of the operator, whence the most powerful remedies had not been able to eliminate them. This discovery was made on the 16th of April, 1852.

The authors employ a pile of thirty pair of plates, approaching, at the same time, that of Bunsen and of Grove, as coke and platina enter into its composition, by which its action is rendered more energetic. Each pair has a diameter of 40 millimetres, and is 217 millimetres high. The number of the pairs to be used at the beginning of the operation depends upon the temperament of the patient and the nature of the malady. Thus a delicate and very nervous person is at first submitted to the action of ten or twelve pairs only, and every five minutes the number is increased. A person of a sanguine or lymphatic temperament is able to endure a greater number of elements. The same observation applies to the quantity of acid employed in the bath, less being required for a nervous than a lymphatic constitution.

The metallic atoms extracted from the body deposit themselves on the whole surface of the tub; but they are more abundant opposite to the part of the body where the metal was lodged. The size of the metallic spots varies considerably; some are microscopical; others have the dimensions of a pea; those of the size of a pin's head are very common. "I have seen," says M. Poey, "after the first bath of a person who complained of pains in the arms, from having taken mercury, the contours of the arm perfectly drawn upon the metallic plate by the deposit of metallic atoms that without doubt proceeded from the suffering member."

We shall terminate our article with an experiment made before the members of the Faculty of Medicine of the Havana.

A patient had undergone during a whole week an external mercurial treatment (frictions with mercurial ointment). He had then taken several lukewarm baths, and it could not be supposed that any mercury still remained attached to the skin.

He was put into a water-bath mixed with muriatic acid. After having remained in it for five minutes some of the water was taken out, and afterwards analysed by M. Baraceca, who found no traces of mercury in it.

The circuit was then closed; and, after the electric current had acted for about an hour, a new sample of the water was taken. Mixed with an alkaline sulphuret, the water became black; and a piece of copper having been dipped into it, gave sure signs of the existence of a small quantity of mercury. Thus the water of the bath now held mercury in solution.

During the experiment, a perfectly clean piece of copper had been placed at the negative pole. When it was taken out of the water, towards the end of the operation, its yellow-greenish colour not only testified an oxidation in which mercury had taken a part, but small white spots were scattered over the surface, one of which, of the size of a square line, was very brilliant, and of a mercurial whiteness. The plate having been heated underneath, the spot disappeared, and the original colour of the copper was restored, which proves that the spot was mercurial.—*La Presse.—Med. Times and Gazette, March* 3, 1855, *p.* 216.

124.—ON THE TREATMENT OF BUBO.

By Henry Thompson, Esq.

Counter-irritation over the swelling, whether threatening to suppurate, or in the act of doing so, has long been employed as a means of discussing the contents of bubo. Especially of late, however, new methods of accomplishing this desirable end have been tried, with variable success. Malplaquet's mode of applying a small blister to the inflamed surface, and dressing it afterwards with a strong solution of bichloride of mercury, for two or three hours, so as to induce a thin slough, has been attended apparently with benefit in some cases. Painting with strong tincture of iodine probably acts favourably, sometimes, on a similar principle, by the extent of inflammation of the skin which is set up, and not from any special power of inducing absorption peculiar to the iodine. The employment of a strong solution of nitrate of silver seems, however, to have afforded by far the best results of any of the methods we have yet had an opportunity of observing. The first systematic trial of this agent was made, we believe, at Guy's Hospital, and with very successful results. Mr. Henry Thompson has subsequently adopted it at the Marylebone Infirmary for a very considerable number of cases, in some of which very marked results have followed its application. In several of these, where fluctuation in the bubo has been so complete and unmistakable as almost to forbid any attempt to discuss it, on the ground of its apparent hopelessness, two or three applications of the solution, at two or three day intervals, have produced diminution in the size of the swelling, increased firmness to the touch, and evident consolidation; while a continuance of the treatment during another week or two has thoroughly succeeded in averting all fear of suppuration, and in dispersing the tumour. Mr. Thompson has applied the same treatment to abscesses of other kinds, when not depending on diseased bone, or in other obviously unsuitable cases, with a very considerable amount of success. In some instances where suppuration and discharge have occurred, there has been a manifest improvement in the condition of the abscess, as an apparent result of the application, and the subsequent progress of the sore has been much more satisfactory than that which these cases commonly present.

The solution which seems to answer best is, three drachms of nitrate of silver to the ounce of distilled water, with twenty minims of strong nitric acid. It may be applied with a glass rod or stick, although the best instrument is probably a glass brush. Freely rubbed over the entire surface of the swelling, and allowed to dry, it produces a thin, black eschar, which begins to peel off in a few days, when the application should be again made, and repeated as often as may be necessary. Where, however, it is more convenient to use the solid nitrate, an application of this to the affected part, previously wetted with a little water, answers every practical purpose. The principal advantage which the solution possesses, arises from the uniform strength which is thus ensured in the application.—*Lancet, March* 31, 1855, *p.* 340.

125.—ON PRESERVED MEAT-JUICE.

By Dr. Robert Christison, Professor of Materia Medica in the University of Edinburgh.

About eighteen months ago, when consulted in the case of a relative of Mr. Gillon, the extensive and skilful manufacturer of preserved meats at Leith, I found that the patient was entirely supported, in a severe illness, by the preserved juice of meat, which had been given at Mr. Gillon's suggestion. Observing the readiness with which it was taken when other food of every kind was refused, I was induced to try it in other instances, and eventually to employ it in various states of disease. The results led me to suggest the use of it to many professional friends, and to advise the druggists of Edinburgh to keep it; so that it is now much in the request, and may be easily obtained.

This substance is the pure juice of beef, preserved in the way in which meats and vegetables are now so extensively preserved in the fresh state, for store provisions. The mode of preparation is as follows: Cylindrical cases of tinned iron are filled each with six pounds and a half of beef; and the lid is soldered on, but with a hole about half an inch in diameter in the middle of it. Two trays of such cases are shoved into iron retorts, analogous in form to retorts for gas-making, but double-cased, so that steam may be introduced into the interstice around. They are thus subjected to a heat of 220° under steam pressure, for about three hours, by which the beef is partially cooked, and, being thus also made to contract strongly on itself, squeezes out a portion of its juice, amounting to a few ounces from each tin. The tins are then drawn, the juice is poured out, and the meat, with certain additions, is subjected to the preservative process. The juice, after being cooled, and entirely freed of fat, is put into small four-ounce tin cases. Each of these has a small aperture at one end, which is secured with solder, after the juice is poured in. The tins are then subjected, on trays, to a temperature of 220° in a muriate of lime bath. On being removed, the solderer rapidly touches with his iron the solder on the top, which giving way allows steam to rush out forcibly and carry with it the air in the upper part of the interior. By the time he has thus swiftly passed over sixteen or twenty tins, the first is ready for being re-soldered by a similar dexterous application of his iron, which then in succession as quickly secures the whole open and steaming apertures. The process of heating in the bath, tapping, and re-soldering, is then repeated a second time, to make sure of the thorough expulsion of every particle of air. The tins finally are painted to preserve them against rust.

The process is most perfect. I have repeatedly opened tins eighteen months in my possession, and stated to have been many months in store when I got them, and in every instance the contents had the

rich delicate aroma and taste of fresh beef-juice. Sometimes the taste is slightly resinous or soapy, in consequence of a little resin having obtained admission in the operation of soldering. But as this does not occur often, the impurity may be avoided with due care. The juice may be taken with relish in small quantity, either cold or warm, in its concentrated shape; but it is rather strong to be used without dilution. When diluted with three times its volume of boiling water, and duly seasoned with salt and pepper, it makes a more palatable beef-tea than any which can be made in the usual way. Sometimes, indeed, a patient will be found to prefer the ordinary sort, either because the preserved juice has unluckily been resinous, or on the principle that leads some people from the plains of England to prefer hard water to the pure mountain springs of the primitive districts of Scotland, viz., because they are not accustomed to the finer sort. But this is not the general fact; and there can be no doubt that the preserved meat-juice makes a most palatable beef-tea, and an equally eligible basis for many soups.

Until about ten years ago, in concurrence with general opinion, I used to regard beef-tea as a highly nutritive article, not to be rashly or freely given during disease. My sentiments in this respect were shaken, when I ascertained, in the course of some experiments for adjusting the dietaries of the General Prison and the Royal Infirmary, that a pint of the very finest beef-tea contained scarcely a quarter of an ounce of any thing but water. Since that time I have much more readily listened to the cravings of patients for beef-tea in even many acute diseases, and above all in protracted subacute diseases, and in chronic diseases with fever; and I have thought I saw that it maintains the strength almost like wine, lessens emaciation and weakness in tedious diseases, and does not occasion any increase of reaction. There is no disease in which these properties are more remarkably shown than in protracted cases of gastric fever, of which, by the way, I have seen an unusual number both in town and country during the last three years. These cases have often lasted for six weeks, or—with a relapse, from too early indulgence or exposure, —for the long term of three months nearly; during which little, or absolutely nothing else, was taken, except beef-tea or diluted meat-juice; and without the attenuation and debility which so protracted a fever and want of appetite ought to have induced. In some instances I could scarcely doubt that life was preserved by this nutriment. It is unnecessary to particularise the various states of disease in which the same practice has been followed. It is peculiarly applicable to all subacute protracted diseases, whether febrile or otherwise; and in all such there is even no great reason to hesitate in resorting to it when local inflammation is present. Every one, I think, will be struck with the readiness with which such patients will often take diluted meat-juice or beef-tea repeatedly when they refuse all other kinds of food. It should be given in the quantity of a teacup-

ful at a time, every four or six hours; but it is well to alternate it with some other simple nourishment, when the patient will consent to do so.

What is its mode of action? Not simply nutrient. A quarter of an ounce of the most nutritive material cannot nearly replace the daily wear and tear of the tissues in any circumstances. Possibly it belongs to a new denomination of remedies, whose action never was even suspected to exist until recently—those which, by some peculiar influence, diminish the waste of the tissues under the exercise of their functions. Professor Lehmann has proved [Annalen der Chemie 1853], that coffee possesses this singular property in so remarkable a degree, that in persons following an active occupation an infusion of an ounce of roasted coffee daily will reduce the daily waste by a fourth part; and the same property seems likewise to belong to tea, and other restorative beverages. It is not improbable that the sapid and saline principles of meat, united in what is called ozmazome, and constituting the ingredients of beef-tea, and meat-juice, possess some such property. It is difficult otherwise to account for the interesting results obtained by the late Dr. Edwards in 1833, who, in his researches on nutrition,—strangely overlooked by the celebrated Gelatin Commission of the French Institute, in their condemnatory report on gelatin in 1841,—found that dogs die slowly if fed on bread and gelatin alone, but, when thus greatly reduced, quickly regain flesh and strength by the addition of two ounces of meat-tea, which cannot appreciably increase their textures by its own insignificant amount of solids. Either it acts as a digestive ferment, so to speak,—promoting the assimilation of other nutriment—or, like coffee, it must lessen the waste of the tissues in the exercise of their functions.

Mr. Gillon's meat-juice contains only $6\frac{1}{2}$ per cent. of solids. As a mere nutrient therefore, it is much in the same category with beef-tea. Sixteen ounces of beef-tea, made with the contents of one tin, yield only 114 grains of solid extract. It contains no fibrin, no albumen, no gelatin. It does not even gelatinise on exposure to the air for days: it is ozmazome, with the salts and sapid and odorous principles of meat, and materially different from all boiled extracts.

I should add, that no good beef-tea can be made so cheap as with this preserved meat-juice. A tin of four ounces makes sixteen of strong beef-tea. This much requires, in the ordinary way, a pound of the finest beef, which at present costs ninepence, and is scarcely ever so cheap as sixpence. The reason for the cheapness of Mr. Gillon's meat-juice is, that the residual meat is economised, while that of the ordinary cooking process is good for nothing.

It is a much more convenient article for use than any of the extracts made from meat by extemporary processes in the kitchen, or

by certain very dubious chemical methods lately come into vogue. It differs materially from all meat-extracts prepared by boiling.—*Monthly Journal of Medicine, Jan.* 1855, *p.* 6.

126.—*On Ammonia evolved in Respiration.* By Professors Viale and Latini.—After a series of delicate experiments, conducted with the greatest care and minuteness, these eminent chemists have arrived at the following conclusions :—

1st. That ammonia is evolved in each act of expiration. They consider that a healthy man expires, in 24 hours, 0.763 grammes of ammonia ; and in a year a quantity equal to 278·504 grammes of the gas. They calculate that in a city containing 160,000 inhabitants, 44·56 kilogrammes of ammonia are evolved by respiration in the course of a year.

2nd. That the ammonia is given off in the form of a sub-carbonate.

3rd. That the lungs cannot expire *pure* carbonic acid gas.

4th. That the *azote*, pronounced by chemists to be a product of respiration, is merely ammonia.

5th. That this ammonia keeps up the supply of the gas existing in the atmosphere, which, dissolved in rain, falls upon and enriches the earth.

6th. That the active principle of contagion is probably merely an ammoniacal salt. Their reasoning on this point is rather curious: —The lungs, skin, and excretions all give off a considerable quantity of ammonia, and contagious epidemics are generally developed where a large number of people are crowded together within a small space, or where the emanations of putrefying animal substances are rife. The latter emanations are always ammoniacal. Fumigations of chlorine, chloride of lime, nitric and acetic acids, are the antiseptics which decompose the ammoniacal salts, and probably also the contagious principle. Heavy rains, and tempestuous winds have been known to arrest the course of epidemics of contagious diseases. Viale and Latini believe that the ammoniacal salts which constitute the principle of contagion may be united either to carbonic acid, or to the metalloids (as sulphur, phosphorus, iodine, &c.), or to animal products, generating cyanogen, or to fatty bodies, fixed and volatile oils, or even to albumen, so as to act as a ferment.

7th. That during respiration a certain quantity of albumen is eliminated.

Professor F. Liebig, of Berlin, has published a letter in which he attaches great weight to the foregoing deductions, and considers the demonstration of ammonia in respired air as a fact of great scientific importance.—*L' Union Médicale.—Monthly Journal of Medicine, Jan.* 1855, *p.* 58.

127.—*Sulphuric Acid, versus Urine*—Mr. James E. Huxley has pointed out that in the wards of insane establishments which are wet and dirty, no scrubbing or scalding will effect more than a temporary improvement, as the floor becomes a reservoir for the perpetual exhalation of the volatile ammonia. But if dilute sulphuric acid be poured over such floors, in the proportion of one ounce of acid to twenty-four of water, and allowed to remain twenty-four hours, a white film (sulphate of ammonia) will form, which may be removed by washing, and will leave the room sweet. This principle is that of converting a volatile into a fixed salt that may easily be got rid of.—*Asylum Jour.—Monthly Jour. of Medicine, Jan.* 1855, *p.* 62.

128.—*Dropping Fluids from Vials.*—Dr. Coale has found a very simple and ready means of dropping fluids from vials. Unless the lip projects very much, the fluid is apt to run back on the neck, and remain attached until several drops have accumulated. Greasing the neck of the vial (not the lip) will prevent this, and give a clean uniform drop.—*Proceedings of Boston Med. Society.—Monthly Jour. of Medicine, Jan.* 1855, *p.* 96.

129.—ON COD-LIVER OIL.

[In an interesting review of several little works on this subject in the 'Edinburgh Monthly Journal' for April, the Editor remarks,]

Thus, chyle is an emulsion formed by the union of oil and albumen derived from the food; and no one can doubt that, out of that chyle or fatty emulsion blood is formed. When, then, after giving the oil to emaciated persons, we observe that they become stronger, more muscular, and fatter, it requires little reasoning to prove that we must have added something to the chyle, through the chyle to the blood, and through the blood to the tissues. Again, when we examine chyle microscopically, we find it to be composed of exceedingly minute particles, or to consist of a molecular basis, which is soluble in ether, and therefore fatty, and that as it flows from the intestines towards the blood, through the mesenteric glands, and, lastly, through the thoracic duct, blood corpuscles may be seen to form in it. Hence, the molecular is a primary and evolving element, out of which the blood corpuscles and the blood itself are evidently formed. This view, modified as to one or more unimportant details, is the one which is in most accordance with physiology, and is most generally adopted.

This theory still finds many objectors, the latest of whom is Dr. Black, who says of it—

"It may be urged that, were fat a 'primary and evolving element,' as considered by Dr. Bennett, it would not be diminished in quantity as it flows along the lacteals; that nature would not thus all but

destroy the very resources whence the nuclei of the tissues are to be drawn; that it would exist in greater proportion in the blood; that the quantity found in that fluid is manifestly insufficient to form with albumen the necessary nuclei for the proper nutrition of the tissues; that it would form a constituent part of healthy muscular fibre, and of other structures having a cellular basis of formation: and that, when found as a constituent part of such tissues, it would not be, as it invariably is, associated with disease. It seems, therefore, more consonant with facts to regard fat, absolutely considered as such, as taking no part in the formation and nutrition of tissues, than to look upon it as a 'primary and evolving element' of such tissues."

Here Dr. Black has evidently misunderstood what is meant by "primary and evolving element." In a note he tells us that he supposes it to mean an element which enters directly into the formation of tissues. But this, so far from being the primary, is really the ultimate element of histogenesis. By the primary element histologists comprehend the first morphological appearance which presents itself, and this is everywhere a mass of minute molecules and granules which, by uniting together, constitute primary nuclei, around which primary cells are formed. All these elements, however, are but temporary, and undergo successive elaborations, evolutions, and reformations, before they directly enter the tissues. The argument advanced by Dr. Black, namely, that fat is not a primary evolving element, because it diminishes as it flows along the lacteals, is exactly that which every histologist employs to show how, by its transformations, it becomes prepared to form tissue. That oil may serve to form albumen, and that albumen may be transformed into oil, nobody can have any doubt who watches in an ovum the development of an embryo. Indeed, although we are willing to pay every attention to Dr. Black's objections, and to give him credit for sincerity in expressing his opinions, it is impossible to overlook the fact, that he is not familiar with the great progress recently made in histology and embryology. He has been led away too far by the hypothesis of germinal centres, and, from what he has advanced, does not appear to us sufficiently to appreciate what may be called the molecular theory of morphology.

Passing, however, from these speculations, it may be asked, how is it that, if cod-liver oil operates in virtue of its oily or fatty matter, that any other oil or fat does not answer the same purpose? Now the fact is, under ordinary circumstances, the stomach and alimentary canal are capable of reducing all kinds of fat to the required fluidity and purity necessary for the formation of chyle. But there are a certain class of cases where the digestive powers are evidently impaired, and cannot do so. We shall not at present enter into the theoretical views by which some have endeavoured to account for the peculiar form of dyspepsia in these cases, because we should thereby be led away too far. We may safely assume that there is such indigestion, and in the vast majority of cases a remarkable antipathy to eating all

kinds of fatty, or, as it is called, rich food. It has indeed been asserted —and we find it is so by Dr. Black—that tubercle frequently occurs without the slightest symptom of a dyspeptic character being present. If by this be understood that individuals who are not habitually underfed (as often occurs among the lower classes) become tubercular, and waste away, whilst the appetite remains good and digestion is perfect, we will venture to say the assertion is altogether opposed to experience. The statements of patients, indeed, would often lead a superficial examiner to conclude that their appetite and digestive powers are everything that could be wished; but we need scarcely say that the hopeful and flattering accounts given by consumptives of themselves are not likely to deceive the sagacious physician, still less to be received by him as an argument for or against any particular theory. Our own notion, therefore, is, that in giving animal oils, we introduce an essential element of nutrition, already so prepared that it is rendered easily assimilable. Of this we are convinced, that it is in proportion to its capacity of being digested, and of forming good chyle, that its good effects are rendered obvious.

But why should cod-liver oil be more assimilable than any other kind of oil? This is a difficult question to answer. The vegetable oils, indeed, are more or less purgative, and have been largely tried, without producing any benefit. Many other animal oils have been employed, and, it is said, with some effect—for instance, neat's foot oil. We have given scate, shark, and dog-fish oil—for samples of which we hereby thank the donors—all with good effect; and we believe that these oils are little, if it all inferior to cod-liver oil. We have also given various kinds of genuine cod-oil, which have been kindly forwarded to us from various parts of this country, and from abroad, and have especially to thank Mr. Archibald of Newfoundland, Mr. Fox of Scarborough, and Mr. Hansard of London, for specimens recently forwarded to us.

We have no difficulty whatever in speaking of the rival claims of the different manufacturers of this substance, because we believe—in accordance with the views previously stated—that they are all good, and are medically of equal value. Yet these oils vary greatly in their physical properties. Here we cannot in justice forbear mentioning that the Messrs. Parker, oil manufacturers in Leith Walk, have for many years—indeed long before Dr. C. J. B. Williams spoke of the advantages of giving a pure oil—manufactured it as pure and tasteless as has been done by anybody. It is unnecessary for them to make their oil known by advertisement, for the simple reason that every drop they can produce is eagerly purchased by the druggists of Edinburgh. They were the first, as they are still among the best and cheapest producers of this substance. To meet the increased demand in England, however, an excellent, pure, and almost tasteless oil has been brought into the market, and that of Messrs. Archibald and Fox have in these respects certainly never been surpassed. Those persons, therefore,

who find, from experience, that they can take with greater ease those light and comparatively tasteless oils, may with great confidence employ any of them.

The notion, however, that a pale oil is superior to the brown kind, is most erroneous. Throughout Germany and the northern kingdoms of Europe the brown oil is preferred, and the assertion so readily made, that its more nauseous smell and taste prevent its introduction into practice, or interfere with its assimilation, is entirely opposed to the most extended series of facts. We can declare, from the result of no small experience, that not unfrequently the brown oil is retained on the stomach, when the light oil is not, and that children more especially, contrary to what may be supposed, in many cases prefer the brown to the more tasteless kinds. Many describe the brown oil as stimulating and acting like a cordial. The conclusion we have arrived at then, with regard to the superiority of one or other kind of oil, amounts to this, that that is the best which is most easily taken, retained on the stomach, and digested, and when we have to do with a capricious organ, experiment alone can decide which that shall be.

It is only right, however, to give Dr. de Jongh's view of the action of the oil, which is as follows:—

"Our analyses have shown that cod-liver oil is a very compound remedy. We find in it fatty matters, biliary principles, iodine, phosphoric acid, phosphorus, butyric acid, gaduine, and several inorganic salts. To which of these principles, many of which are recognised as very active remedies, does cod-liver oil owe its salutary action in rheumatismal and scrofulous affections? Is it to the iodine, to the fatty principle, to phosphorus, to other matters, or to the action of all these principles united? If we consider that, in the diseases in which cod-liver oil exhibits an incontestible value, the digestion is augmented, the nutrition ameliorated, the secretions excited, the function of the lymphatic system revived, and lastly, that the ganglionic system is affected, we shall be easily convinced that it is not the biliary principles, not the fatty matters, not iodine, or any other single one which can meet all these indications. Hence why we attribute the efficacy of cod-liver oil, as much to the combined action of all these principles as to that of many among them."

This is certainly an ingenious way of avoiding a difficulty. Dr. de Jongh avows his want of power to resolve the question of how one or more of these principles act, and evidently regards cod-liver oil as a mixture made up of certain precious ingredients, each of which has its destined, though undiscovered object, while the whole of them unite in producing a given effect. Hence, only the oil that contains all these ingredients is a good oil, and as they are present in the brown kind as determined by analysis, this is the oil to be preferred.—*Monthly Journal of Medicine, April* 1855, *p.* 317.

130.—*Phosphate of Lime.*—Dr. KUCHENMEISTER recommends the following formula in cases in which phosphate of lime is indicated:—Calcis phosphat. ℥ij.; calcis carbon. ℥j.; sacch. lactis, ℥iij. M. ℥ss. bis terve in die. Instead of the milk sugar, lactate of iron may be substituted, if iron be required. The especial use of the carbonate of lime appears to be that carbonic acid is liberated by the acid of the stomach, and dissolves the phosphate. Lactic acid also is formed from the sugar, or is set free from the lactate of iron, and dissolves the phosphate. The most ready way of absorption is, however, when the phosphate is given with food, especially with milk, with which it forms a soluble combination.—*Schmidt's Jahrb.*—*Monthly Journal of Medicine, April* 1855, *p.* 345.

131.—*On the addition of Tannate of Lead to Adhesive Plaster.*—M. Herpin states, that his own experience teaches him that Bayntón's treatment, by strapping, may be extended to every branch of surface, whether resulting from wound or ulcer; and the only inconvenience he has found attending it is, the production of eczematous eruptions, or vesications in irritable skins. After having tried various means of obviating this he remembered the great advantage that accrued from the treatment of bed-sores, by means of plasters powdered with the *tannate of lead.* He caused some of this substance to be combined with adhesive plaister, which henceforth produced no irritation. As the addition of the tannate diminishes the adhesiveness, the proportion may vary accordingly, as this quality is desired or not. It is retained when 1-20th of the tannate is added, and when not much required the proportion may be raised to 1-12th.—*Bullet. de Thérap.*—*Med. Times and Gazette, April* 7, 1855, *p.* 346.

132.—*Gelatine Paper.*—Mr. DOBELL has called the attention of the Royal Society to Gelatine Paper as a medium for colouring light, likely to be useful in many employments, and in cases of weak sight. This kind of paper, which was first invented at Rouen, in 1829, is now produced in great perfection; it is highly transparent, and in sheets measuring sixteen inches by twenty-two, but can be made, if required, of the dimensions of the largest plate glass. These sheets, moistened with a solution of gelatine, may be stuck on the panes of a window, and thus change the light admitted to any required colour. A green light, falling on the white silk made up by dressmakers, deprives it of all its painful glare; and in the same way, yellow silk is made to appear green by a blue light, as has been proved by actual experiment, and it is attended with the happiest results. Jewellers who have tried the green paper, say, that when once accustomed to working in a coloured light, they find it greatly relieves their eyes. In reading, too, a sheet of the green paper laid on the page preserves weak eyes from being

injured by the strong contrast of black and white, and enables many to read with comfort who have been hitherto obliged by too susceptible vision to abstain from books. Other applications of gelatine paper naturally suggest themselves; it may be used as screens and shades for many purposes; the glasses of spectacles may be coated with it; gardeners may use it in their conservatories; and the yellow will probably be taken into their service by photographers. By the addition of a small quantity of acetate of alumina during the process of manufacture, the gelatine paper becomes waterproof, just as linen or woollen cloth is rendered waterproof by the same chemical substance.—*Chambers's Journal.—Med. Times and Gazette, Jan.* 6, 1855, *p.* 23.

133.—*Chlorate of Potass in Mercurial Salivation.* By MM. Herpin and Blache.—Encouraged by the success of Hunt, and others, in the employment of the chlorate of potass in the treatment of gangrenous affections of the mouth, M. Herpin has repeatedly employed it with the best effect in mercurial salivation, giving it in the dose of ℥ss. to ℥i. per diem—the cure, when the case is taken early, being completed by the fourth day. He has long been accustomed to the employment of this substance, as recommended by Odier, in jaundice, whether simple or complicated, with engorgement of the liver. Odier carried the daily dose as high as 150 grains.

M. Blache also furnishes us with the results of his experience with this substance at the Hôpital des Enfans. He found that, while cases of *ulcero-membranous stomatitis*, treated by the fuming chlorhydric acid, or the chloride of lime, required a mean duration of treatment of twenty days, those treated by the chlorate of potass were quite cured in five or six days. In *mercurial stomatitis* he has also found it highly serviceable. He gives from 30 to 60 grains, in a gummy julep, in the twenty-four hours—a small dose, indeed, compared to the 300 or 450 grains prescribed by M. Socquet, of Lyons, in acute rheumatism.—*Bulletin de Thérap.—Med. Times and Gazette, March* 24, 1855, *p.* 289.

134.—*Etherial Solution of Nitrate of Silver.*—Every one who has tried it is aware that the application of a watery solution of the nitrate of silver over a large extent of skin is a troublesome and patience-requiring process. The fluid does not dry quickly, and it runs about, being prevented, by the greasiness of the skin, from being rapidly absorbed. A plan which we see adopted by Mr. Ward in the London Hospital obviates very completely these difficulties. It consists in making the solution with the common nitric ether, instead of water. The ether acts as a solvent of any sebaceous matter which may be on the skin, and, from its volatility, very quickly dries in, producing, at the same time, a sensation of coolness very agreeable to the patients. If wished, several coatings may be applied successively to the same

part with loss of but little time. The strength which Mr. Ward generally employs is eight grains to the ounce, but it may, of course, be modified according to the wishes of the surgeon. The use of nitrate of silver externally in erysipelas and other low forms of inflammation of the skin, is a very favourite practice in the London Hospitals,—either an aqueous solution, or the solid stick moistened being usually employed. Mr. Ward informed us, that the use of the etherial menstruum was not original, but had been suggested to him by a gentleman by whom he had been consulted.—*Dublin Hospital Gazette*, *Nov.* 15, 1854, *p.* 320.

135.—ON A PRESERVATIVE MEANS AGAINST THE SYPHILITIC VIRUS.

By M. A. Rodet.

[*Syphilization*, as it is called, has failed as an antidote to this poison, so also has *syphilitic vaccination*. M. Rodet thought that some neutralising liquid might be discovered.]

For some time I had been occupied in watching the effects produced by different chlorides employed in the dressing of chancres and ulcerated buboes, and I remarked, that, of all remedies, the chloride of zinc possessed the most remarkable properties. Dissolved in thirty to forty times of distilled water or alcohol, it powerfully modifies the surface of chancres, transforms them rapidly into simple sores, especially when elevated, and induces a cicatrisation in a very few days. It has no effect on the skin when the epithelium is intact, and it passes through the least fissures.

Dissolved in eight times its weight of distilled water, and applied to a puncture recently inoculated, it destroys the virus and prevents the formation of a chancre; but, as it slightly cauterises the interior of the puncture, there forms around, at the end of two or three days, a slight inflammatory zone, whence results a simple pustule, which ordinarily lasts six to nine days.

The chloride of zinc, therefore, does not fulfil all the desirable conditions; it protects, but it cauterises.

The iodide of zinc, the chloride of cadmium and barium produced analogous effects; they protect, when the solution is sufficiently concentrated, but they give origin to simple pustule. The chloride of iron does not cauterise the punctures, but it does not protect, nor does it ever retard the progress of the virus. Whatever may be the amount of the concentration, no other result is obtained. The preparation, however, appeared to me to be valuable, and I therefore endeavoured to give it, by combination, those properties which it did not naturally possess. After some experiments, the problem seemed to be solved. Having applied to an inoculated puncture a solution of perchloride of iron and citric acid, protection was obtained in a most undeniable

manner. I repeated the experiment a certain number of times, and always with the same result, when I was suddenly stopped by one of those unforeseen occurrences which disturb most scientific investigations.

The sample of perchloride of iron which I had hitherto used being exhausted, I procured another, which I found not to possess the same properties. No preservation was obtained, neither with the same nor with stronger doses. I tried in vain to procure a specimen similar to the first, and, from constant failure, was beginning to doubt the accuracy of my first observations, when I discovered two little vessels of the original perchloride in my cabinet. The results here obtained were precisely the same as at first.

I now proceeded to investigate the nature of the differences between the two sets of preparations. The perchloride first used was quite soluble in distilled water; the others threw down a deposit. By adding to the new samples a sufficient quantity of hydrochoric acid to render them completely soluble, a fluid was produced exactly similar to that which had in my early experiments proved so efficacious, and therefore it was necessary to determine—

1. What are the doses and the compositions which preserve the best without irritating the sound tissues?

2. What is the most simple and efficacious manner of employing the remedy?

3. What are the appreciable effects upon the punctures of inoculation?

4. What is the duration of time during which the remedy possesses a preservative power, and what are the modifications which its effects produce at different periods from the insertion of the virus?

5. What are the causes which make the preservative effects of the remedy vary?

6. What are the other applications of which the remedy is susceptible?

From the very numerous experiments which have been instituted, the following propositions are laid down. The best formula is,—

Distilled water..... 32 grammes = ℥j. +℥iss.
Perchloride of iron
Citric acid ..... } aa. 4 grammes = (approx.) ℥j.
Hydrochloric acid

A similar fluid may be made by omitting the citric acid and augmenting the dose of the hydrochloric acid one-third. The most simple method of employing the remedy consists in letting fall one drop upon the part inoculated by the virus, and in letting it remain there ten or fifteen minutes; or by applying to the part a piece of lint soaked in the fluid. If the contact of the liquid is of too short duration, the preservation is incomplete, and an ulcer forms, which advances slowly, and may be regarded as an imperfectly-formed chancre.

If the lint or charpie be applied for the space of one hour the

preservation is complete. A shorter time might suffice, but no inconvenience would result if the fluid were allowed to remain twenty-four hours.

At the moment of application the patient feels a smarting sensation, which lasts but an instant; a minute afterwards the puncture becomes raised so as to form a pimple, and spreads from the centre to the circumference, so as to resemble a gnat bite. In about half an hour it ceases to extend. In two hours it begins to disappear, and shortly afterwards there is no trace left. This elevation is a certain proof that the fluid has penetrated the puncture, and permeated the subjacent tissues. Preservation may be obtained even when the virus has produced no appreciable effect upon the spot where it has been applied. At the end of two, four, or six hours, its effects are as complete and lasting as if it were applied immediately after inoculation. Should the inoculation have produced sensible effects, such as the formation of a pimple or of a pustule, the absorption of the liquid goes on badly; consequently, the patient is imperfectly protected. Cauterisation with a piece of nitrate of silver is then the only sure method. The effects of the preservative liquid may be modified, not only by the doses of the active substances which enter into its composition, and by the duration of its contact with the contaminated parts, but also by the degree of activity of the virus employed. Weak doses have preserved in some instances, and in others produced unsatisfactory results. Hence it may be inferred, that the syphilitic virus is the more energetic in proportion as the ulcer which it produces is more recent.

The liquid acts beneficially upon simple ulcers by causing a puriform secretion when deficient.

The vaccine virus is completely neutralised by this liquid, and perhaps its opportune application to those parts of the body where cicatrices are a source of disfigurement in small-pox, may be attended by a similar good effect; the poison may be neutralised and the progress of the pustule stopped.

Finally, can this liquid neutralise the poison constituting the disease known as hydrophobia as well as that of syphilis or small-pox? If experience should reply in the affirmative, science will have made an important step. As the remedy does not cauterise the tissues, it may be freely applied to the bitten parts, even in event of there being a shadow of suspicion that the animal was diseased. Cauterisation is at the best a painful mode of treatment, from which people shrink in terror. To be effectual it must be very extensive, and it has failed even when applied by experienced hands.

In thus making public a mode of neutralising the syphilitic virus, I have performed a sacred duty. I trust that it may be permitted me to follow up its application, and to be the means of throwing a veil over one of the most hideous sores of society. Should it prove a successful plan, and become received with favour, I fancy that I hear

already from afar the murmur of the reproach of immorality. Should such a cry be raised I shall rebut it with all the energy in my power. That which is immoral is the debauch, depraved habits, promiscuous intercourse; it is, in short, all that can render necessary the employment of prophylactic measures. It is an act of immorality on the part of the surgeon to hold in his hands the means of preventing a great evil, and from some motive to neglect to use it. Medicine resembles charity; it should do good with the head turned aside; its sacred duty is to cure evils, from whatever source they emanate, and to protect mankind as well from syphilis as from any other scourge.—*Gazette Hebdom., Jan.* 12, 1855.—*Med. Times and Gazette, Feb.* 10, 1855, *p.* 142.

136.—*New Broth for the Sick.*—[Dr. Thudicum exhibited before the Medical Society of London, a new broth for the sick.]

To prepare this broth, half a pound of the flesh of a recently-killed animal (beef, or the flesh of a fowl) is chopped fine, and well mixed with a pound and an eighth of distilled water, to which four drops of pure muriatic acid, and from half to a drachm of common salt, have been added. After an hour, the whole is thrown on a common hair sieve, and the fluid is allowed to run off without pressure. The first portion, which is turbid, is poured back, until the fluid runs off quite clear. On to the fleshy residue in the sieve half a pound of distilled water is thrown in small portions. In this way a pound of fluid (cold extract of meat) is obtained, of a red colour, and an agreeable taste of broth. The sick are allowed to drink a cupful cold at pleasure. It must not be heated, as it then becomes turbid, and deposits a thick coagulum of animal albumen and hæmatin. The broth possessed great advantages over other preparations of meat, from containing albumen, and being remarkably easy of digestion. Cases were referred to in which its efficacy in this respect had been tested. Some conversation took place on the advantages which this broth possessed over concentrated beef-tea, &c.—*Lancet, Dec.* 16, 1854, *p.* 508.

137.—*On Collodion.*—Different proceedings have been instituted to give to collodion a greater amount of elasticity, with or without increasing, by the addition of certain substances, its therapeutic properties. Although experience has not yet pronounced upon the value of these trials, we give the principal forms of preparation.

1. *Elastic Collodion.*—Three formulæ are indicated: the first is due to M. Robert Latour, who employs impermeable coverings for the cure of local inflammations.

Collodion ℥iss., Venice turpentine gr. iii.; castor-oil ℥ii.

The fluid must be spread in a tolerably thick layer, by means of a brush, over the affected part.

A simpler but less solid form is the following:—

Collodion, 40 parts; castor-oil, 5 parts. Mix.

The castor-oil may be replaced by glycerine.

M. Semoine (Bulletin de Thérapeutique) advises the employment of collodion and bird-lime *(collodion à la glu)*. The difficulty is, however, to obtain the lime in sufficient purity.

2. *Medicated Collodion.*—The good effects following the application of this substance to affected parts of the skin has induced M. Aran to recommend—collodion, 100 parts; tinct. of the perchloride of iron, 100 parts. Mix.

Dr. Hannon, of Brussels (Presse Médicale, 1850) recommends the combination of collodion with the acetate of lead, by pouring, drop by drop, an ounce and a-half of collodion into warm concentrated alcoholic solution of neutral acetate of lead, continually stirring up the mixture. The saturnine collodion is very supple; and, according to the ideas of the author, participates in the properties of the two principal constituents. It seems doubtful whether these mixtures possess qualities superior to simple collodion.—*Archives Gén.—Med. Times and Gazette, Dec.* 9, 1854, *p.* 597.

138.—*External Employment of Aconitine.*—[In a case of painful stump, under the care of Mr. Hilton, in Guy's Hospital, this remedy was found useful.]

Ever since the amputation he has suffered most severely from neuralgia of the ulnar nerve, which has quite prevented him from using the stump. The pain was also increased by the least motion. Mr. Hilton ordered the application of an aconitine ointment over the course of the nerve. The ointment consisted in two grains of the alkaloid to an ounce of lard. Very great benefit speedily resulted, and when the man left the hospital a short time afterwards, the pain had been so completely relieved, that he could use his arm most actively without producing any uneasy sensation. The cure, however, was not permanent, and a month after his discharge the man was re-admitted. Mr. Hilton did not think it advisable to trust him to use the ointment continuously, and as nothing less seemed to keep the pain in abeyance, it was decided to excise a portion of the ulnar nerve. The operation was performed last Tuesday, a little below the elbow, an inch and a quarter of the nerve trunk being cut out. The case is of value, as showing what the effects of the ointment might be in cases in which the neuralgia was due to a transitory cause.—*Med. Times and Gazette, Dec.* 16, 1854, *p.* 614.

139.—*On the Immediate Cause of Coma and Insensibility.* By Dr. Snow.—In a paper read before the Physiological Society, Dr. Snow said that these phenomena were met with under a variety of circumstances; but that they always, in his opinion, were caused by the in-

terruption of the process of oxydation of the brain and nerves, which was necessary to consciousness, sensibility, and all the other animal functions. The effect of respiration, which was a process of oxydation, was very evident on the fœtus, the moment it began to breathe the air, in exchange for the imperfect oxygenation of the blood previously effected through the placenta. In asphyxia, the privation of oxygen caused coma and insensibility, whilst the heart still continued to beat for a time. Narcotics produced coma and insensibility by diminishing oxydation, through a counter affinity which they possessed for the oxygen, as he had endeavoured to show on a previous occasion. The following were the chief facts, which proved that narcotics acted by diminishing oxydation in the system. He had found that the quantity of carbonic acid given off from the lungs was diminished during the influence of chloroform and ether. Bocker had found that the quantity of all the constituents of the urine was diminished by alcohol and by some other narcotics. The temperature of the body, which always bore a direct relation to the consumption of oxygen in the system, was lowered during the insensibility caused by narcotics. Narcotics had most of them the power of preventing oxydation out of the body. Putrefaction was a process of oxydation in its commencement; and nearly all narcotics were antiseptics, their antiseptic power usually bearing a direct relation to their power as narcotics. The vapours of volatile narcotics also had the property of preventing that kind of oxydation which constitutes ordinary combustion. Some parts of the nervous system seemed to have a greater affinity for oxygen than others, as they were able to perform their functions with a less amount of that gas. It was on this account that patients were able to exist in a state of coma and insensibility without dying. In privation of air, and also under the influence of narcotics, the functions of the cerebral hemispheres, and of the nerves of common sensation, were first suspended, coma and insensibility being induced, whilst the respiratory movements and the action of the heart continued. By further privation of air the respiratory movements ceased, from the functions of the medulla oblongata and the nerves of respiration being suspended, whilst the heart continued to act; showing that the ganglionic system of nerves continued to perform their functions. Under the influence of narcotics, also, the action of the heart usually survived that of the muscles of respiration, when these agents were not very rapidly introduced into the circulation. The circulation of the blood was of course necessary to convey oxygen to any part of the body; and it was by the interruption of the circulation in the brain that the diminution of oxydation was produced which caused the coma of apoplexy and epilepsy. The symptoms of apoplexy arose sometimes from extreme congestion, and sometimes from anæmia of the brain, but in either case the circulation was much interrupted; whilst in effusion the yielding coats of the vessel were the first to feel the pressure.

Mr. Headlam objected to Dr. Snow's views, as being more complicated than the simple view of a direct action on the nervous system. In some cases where narcotics were exhibited there was excitement and delirium, in lieu of coma. The diminished oxydation present during narcotism might only be coincident with, and not a cause of the comatose condition.—*Lancet, Dec.* 23, 1854, *p.* 530.

140.—EXTERNAL USES OF THE ACID NITRATE OF MERCURY.

A solution of the nitrate of mercury in strong nitric acid is in very common use at the Hospital for Cutaneous Diseases, and constitutes a very convenient form of caustic. Its formula is—℞. Hydrargyri ℥j., acidi nitrici (specific gravity 1·50) ℥ij.; solve. The solution produced is a clear, colourless fluid. The following may be mentioned as some of its chief uses.

In Carbuncle.—Mr. Startin usually applies the caustic if the carbuncle be of not more than moderate size, to but one central spot, where it is freely painted for an extent of about a shilling in size. Its effect is to produce an eschar, from beneath which the core afterwards escapes.—[See page 265 of this vol.]

In Acne.—A very minute drop of the acid is placed, by means of a finely-pointed glass brush, on the apex of any indolent tubercles, whether suppurated or otherwise. It has the effect of opening the pustule, if matter have formed, and if not, induces the disappearance of the induration. The application is followed only by a little smarting pain, and if it have been carefully made leaves no scar. For an account of the constitutional and other measures which are adopted simultaneously in the treatment of this disease, the reader is referred to the 'Med Times' for Oct. 14, p. 394.

In Boils.—There can, we think, be little doubt as to the superiority of the caustic treatment over that by the knife, even in the case of very large boils. The pain of the incision, the large sore caused, and the unsightly scar which follows, constitute very formidable drawbacks to a practice for which there is no real necessity. At this hospital, where cases of boils are very common, the knife is never resorted to. The general treatment consists in giving aperients and steel conjointly, and the local in applying to the apex of the furuncle a full-sized drop of the acid nitrate solution. The morbid action generally terminates coincidently with the application, and the core is thrown off through a comparatively small opening, the resulting cicatrix being insignificant.

In Lupus.—The acid nitrate is one of the most efficient and convenient forms of caustic in this disease. Mr. Startin does not, however, employ it solely, but uses also the biniodide of mercury, and a paste of which arsenic is the principal ingredient.—(See page 11, for

July 1). The acid nitrate is chiefly used in indolent tubercles, and to indurated patches not actually ulcerated. After ulceration has occurred the arsenical paste is preferred.

For Sloughing Ulcers.—The practice of treating unhealthy ulcerations wherever situated, by means of caustics, is much pursued at this hospital, and with excellent results. The pain attending the application of nitric acid has been much overrated by the profession generally, and its use has consequently been avoided in many instances in which it would have been efficient to completely change the course of the morbid action and induce healthy processes. Its powers in cases of phagedæna are now widely recognised, and its use will probably soon extend to various other kinds of ulceration of somewhat similar nature, but much less severity. The pain spontaneously caused by an unhealthy sore during a single night is probably much more than that produced by an application of caustic. In most cases of sloughing or unhealthy ulcers, Mr. Startin employs either the solution of the acid nitrate or the arsenical paste just referred to. The rapidity with which the surface granulates afterwards is often surprising.

In Moles, Nævi, &c.—Small moles on the face, if superficial and not too thick, may be readily destroyed by the acid nitrate. A cicatrix of course results, but it is small, and far less unsightly than the original disease. Small cutaneous nævi are often treated both at this and the various other London hospitals, by means of the nitric acid. Unless the disease be of very small extent, the employment of a ligature appears to be a much more certain means of effecting the end desired. If there be a subcutaneous base to the morbid structure it often persists in growing, despite frequent applications of escharotics. There is a mild form of dilated cutaneous capillaries which produces the marks known as "port-wine stains," "spiders," &c., in the treatment of which much benefit may be obtained by the dextrous application of fluid caustics. With a finely-pointed glass brush, charged either with nitric acid or the acid nitrate of mercury, the tortuous vascular trunks should be severally painted, a minute streak of the caustic being thus left along the whole course. In this way, by repeated applications, the whole of the larger vessels may be destroyed, and the disfigurement, to a large extent, diminished. The "port-wine stain" is of course very much more difficult to remove than the less diffused forms of this condition, such, for instance, as are of frequent occurrence on the cheeks or nose; even in it, however, much benefit may by patient treatment be gained.—*Med. Times and Gazette, Jan.* 6, 1855, *p.* 9.

141.—*Therapeutic Action of Lupulin.* By Zambaco.—Debout's observation on the favourable influence of lupulin in quieting painful erections induced the author to institute some experiments with the

medicine, which, to prevent mistakes, he administered to the patients with his own hands. The form was threefold; pure lupulin, the tincture, or mixed with an equal part of sugar. The doses were different: of pure lupulin, 1—16 scruple (these high doses caused no disturbance of the nervous system;) the tincture, 2—16; the mixture with sugar, 2—3. From eight well-marked cases of painful erection, consequent on gonorrhœa, out of a much larger number, the author was enabled to draw the following conclusions:—Lupulin possesses a remarkable effect upon the genital organs; the erethisms were quieted in four-fifths of the cases. It is of therapeutic utility when there exists the necessity for keeping the penis in a state of rest. Camphor given in large doses, with a similar object, both irritates the alimentary canal, and often fails in its end. The sedative and anti-blenorrhagic influence of lupulin depends upon a resinous, ethero-oily principle; while the bitter principle yields a real tonic. The author has seen lupulin given with good effect in scrofula.—*Med. Times and Gazette, Feb.* 3, 1855, *p.* 118.

142.—MOVABLE TUMOURS OF DOUBTFUL NATURE.

[We sometimes feel a kind of gratification when we find the eminent men in London puzzled with a case, knowing that we have so often found ourselves in the same predicament. It is very consolatory in misfortune to find ourselves in respectable company—with these feelings we give the following cases:]

Case 32.—*Hard, Movable Tumour the Size of a Fist in the Right Hypochondrium, remaining without Alteration for Seven Years.*—A healthy-looking young woman, aged 28, unmarried, was under the care of Mr. Paget, in St. Bartholomew's Hospital, in Sept. 1852. In the left side of her abdomen, immediately beneath the border of the ribs, was a hard lump about the size of a large fist. It was firm, obscurely lobulated, and not very accurately defined. By pressure, it was easily forced deep into the abdomen, or pushed either downwards quite into the pelvis, or upwards under the ribs, none of which movements occasioned the slightest pain. When the hand was removed, it always rose to its former situation, which was apparently in close apposition with the abdominal parietes. The patient had known of its existence for five years, during which period it had remained stationary as to size, and other characters. She suffered only from dyspeptic symptoms. The case is detailed in the Med. Times for December 25, 1852, page 642. Since that date, we have repeatedly seen the patient, the last time being a few months ago. The tumour has remained precisely *in statu quo*, and, excepting some chlorosis and palpitation of the heart, the woman continues in good health. At St. Bartholomew's, the case has been examined by many surgeons; but no opinion as to the nature of the tumour has been hazarded.

Case 33.—*Hard, Movable Tumour the Size of a Fist in the Right Hypochondrium, remaining without Alteration for Seven Years.*—A delicate-looking woman, aged 38, has attended the City Hospital for Diseases of the Chest, under the care of Dr. RISDON BENNETT and Dr. BIRKETT, at different periods for the last three years. She suffers from frequent attacks of bronchitis, but the main interest of her case consists in the presence of a tumour in the abdomen, very closely resembling in size, in shape, and hardness, a small orange. In September, 1852, it was so freely movable, that it could, without difficulty, be carried from the left side of the umbilicus into the right iliac region, thence upwards into the right hypochondrium, then across the hypogastrium, to be ultimately lost beneath the margin of the ribs on the left side. On being left for a few minutes, the mass always rose to the position it had at first occupied. It was not in the least tender. When examined in October, 1853, it was found to be movable in an area of about two inches around the umbilicus, but not further; its size, &c., were just as before. The woman is at present attending the Hospital, and during the last year no alteration in the condition of the tumour has taken place. She is not in materially worse health than she was three years ago. The history of the existence of the tumour now extends over a period of twelve years, during which it has not increased in size. For further details see the Med. Times for December 18, 1852, p. 621.

Case 34.—*Large, Firm, Movable Tumour in the Right Side of the Abdomen, with History of Existence for Ten Years, and much Increase of late—Patient in Perfect Health.*—In October, 1853, a girl, aged 20, stout, florid, and in apparently good health, was under the care of Dr. WALSHE, in University College Hospital, on account of a tumour in the right side of the abdomen, having the following characters. When placed on her back there rose to the right of the umbilicus a very visible prominence, the elevation of which extended transversely four inches, and from above downwards three and a-half. The skin above it was flaccid, loose, and easily pinched up. On applying the hand, a circumscribed tumour was felt, obscurely divided into two large lobes. It seemed to lie close to the abdominal wall, to which, however, it did not adhere. In all parts it was equally firm and inelastic, and nowhere yeilded any sense of fluctuation. Gentle handling gave no pain, and by pressure the mass might be carried either upwards or downwards, to the extent of two inches, and across the abdomen, to the extent of at least six. It could not, however, be pressed backwards. In the erect posture it descended considerably lower than in the recumbent. On percussion over it a dull sound was elicited, and its size so defined was six inches by four. Between the upper boundary and the liver was a space in which a finger might be laid, and where the percussion note was moderately clear. In the infra-axillary region no interval could be made out between it and the liver. The iliac fossa and hypogastrium were extra resonant; and

posteriorly, over the region of the right kidney, the extent of dulness was not increased, being, if any thing, rather less than on the left side. It did not appear that the functions of any part of the abdominal viscera were in any way interfered with. The history given was, that the tumour had existed for seven years, had been almost painless, but during the last ten months had rapidly increased in size. Dr. Walshe inclined to the diagnosis of a fibrous growth in the great omentum. The patient remained under care for about a month only, and then returned to her home in the country, no alteration in the disease having been perceptible. See 'Med. Times' for Dec., p. 621.

Case 35.—*Fatty Tumour in the Omentum the Size of a Fist.*—A middle-aged man, under the care of Mr. Lloyd, in St. Bartholomew's Hospital, December, 1852, died after the operation for inguinal hernia. He was moderately stout, but not very fat. At the operation intestine only had been found in the sac. At the *post-mortem* there was found at the free border of the great omentum, near its middle, a well-circumscribed mass of consolidated and firm fat, about the size of a fist. Its neck was somewhat constricted. The explanation which seemed most probable was, that at some former period this portion of the omentum had been incarcerated in the hernial sac, and that subsequent to its return it had been the seat of fatty deposition. It had not any appearance of having been at all recently strangulated; and localisation of the fat, which, in other parts of the omentum, was not very abundant, seemed to show that the form of the tumour did not result merely from compression, but that there had been growth of the fatty tissue. The tumour had not been sought for during life but had it been discovered, it is evident that it would have afforded a very puzzling example of a freely movable mass in the abdomen. The Hunterian Museum contains specimens of fatty tumours in the omentum of a cat; but we are not aware of any example on record, excepting the above, of the like condition in the human subject. We narrate the case here, not because it is to be supposed that any one of the three preceding ones resembles it in nature, but on account of its important relation to them in respect to its being seated in the omentum, and being freely movable.

[The next case was that of a woman, aged 22, under Dr. Budd, as King's College Hospital.]

Jan. 21. She is a married woman, and was confined of her first child in Sept. last. She recovered fairly, and for the first two months suckled her infant. About seven weeks afterwards she had an illness which lasted a week, and of which the most prominent symptom was a tendency to rigors and irritability of the stomach, with occasional vomiting. On becoming convalescent from this, she noticed accidentally that there was in the right side, which had never been the seat of pain, a large hard lump. The tumour, soon after its discovery, became the seat of uneasy sensations, and, as she thought for the first few

weeks, somewhat increased in size. More recently, however, she has not noticed any enlargement of it, but for three weeks past there has been more of pain than there was previously. There is at present, however, no pain, and but very little tenderness in it. It is situate in the right hypochondriac and upper part of the right iliac region, and feels as if about the size of an infant's head flattened and moulded against the abdominal wall. It is firm and resisting, without the least feeling of fluctuation. Its boundaries are well circumscribed, and its borders feel much thinner than its middle. Percussion over it is quite dull, unless made by a sharp tap, when a clear note from intestine beneath may be elicited. The mass descends and rises with the respiratory movements, and may by manipulation be made to change its position considerably from side to side. Its borders on the right side, as well as in other directions, can be plainly felt. The woman has a clean tongue and a quiet pulse; her appetite is good, but she complains of thirst, and of some pain in the right lumbar region. Slight rigors, followed by sweating, occur occasionally. The urine is very acid, and contains, as shown by the microscope, oxalate of lime crystals, and a few pus cells. There is some irritability of the stomach, and vomiting sometimes occurs.

This patient remained under Dr. Budd's care from Jan. 21st to March 2nd, during which time no change in the character of the tumour took place, nor did it perceptibly increase in size. The urine was frequently examined microscopically, and, in a general way, contained no pus, nor was it in the least albuminous. The treatment was directed to the relief of the tendency to vomiting, and to perspiration, which having been achieved, the patient was allowed to return home.

Comments.—The last case which we have mentioned very closely resembles Cases 32 and 34, excepting in that a much shorter history was assigned to it, and that there had been and were some constitutional symptoms which might seem to indicate the existence of local inflammation. The characters of the mass, however, were so unlike those of an accumulation of inflammatory product, that it would probably be safer to refer those symptoms to the irritation of some adjacent organ than to the tumour itself. Not improbably, the right kidney was pressed upon,—a conjecture that the presence of pus in the urine, which was at one observation thought to be the case, would go to confirm. As to the date assigned to the tumour, also, it is very possible that the woman was much mistaken, more especially as it had, when first discovered, very nearly, if not quite, attained its ultimate dimensions. If, then, we may venture to regard this as one of similar nature with the three preceding cases, Nos. 32, 33, and 34, we have a series of four cases in which movable tumours existed, superficially placed in the abdominal cavity. In all, the patients were women below middle life, and in three under the age of 28. In three the tumour lay to the right side. In none were there any indications of

malignant cachexia; and in three the account of the existence of the tumour for many years quite negatived any supposition of its cancerous nature. In but one was there anything in the history of the case suggestive of an inflammatory origin. That the tumours were not of an hydatid nature seemed probable from their not possessing a globular shape, and also from the absence of any degree of fluctuation. In regard to the influence of sex, it is probable the occurrence in females of the whole of those we have been able to adduce from recent hospital experience, is quite accidental. Mr. Paget has mentioned to us a case as being the precise counterpart of the one under his care, excepting that the tumour was not quite so large, and the patient was a man. It came under his notice in private practice, and has been repeatedly seen during several years, the patient, meanwhile, retaining perfect health, and the tumour not altering either in size or other character. Its history extends over a long period of years, during which no growth has been noticed.—*Med. Times and Gazette, Nov.* 4, 1854, *p.* 469.

143.—ON ANTISTASIS OR COUNTERACTION.

By Walter Cooper Dendy, Esq., Surgeon to the Royal Infirmary for Children.

If we analyze the physiology of the living world we shall find it displaying a series of antagonisms: the entire process of carrying on vitality indeed may be termed the resistance of one thing to the influence or agency of another. In pathology it is essentially so. We daily see one disorder disappearing as another is developed or established.

In therapeutics, above all, whether their action be similia or contraria, many of the most potent remedial agents effect their influence by some impregnation of the blood with their own especial quality; thus poison and antidote, disorder and remedy, are often but the relative terms for two exciting elements—the one, it may be, perilous, destructive, permanent; the other, salutary and transient. It is probable indeed that something abnormal, some systemic change, is induced somewhere by every remedy in imparting its counteracting influence. The term antistasis indicates in its comprehensive sense this power, which we recognise as antagonistic force, vicarious action, metastasis, specific or special remedy, prophylaxis, vis medicatrix; nor is it confined to physics, for mental influence or derivation will often prove remedial after therapeutic agency has failed. Spontaneous or natural antistasis the controlling or preventive force of one disease over another, is seen in the prophylaxis of measles by small-pox, of phthisis by ague, &c.

During September last I was called to an infant, a year old, who is now convalescent of pertussis. When the cough was at its height, it

was at once and entirely checked by a sudden attack of spasmodic cholera. On its recovery from this, the hooping-cough re-assumed its severity. Had this been a case of common inflammation, the flux might probably have cut it short at once; the *special* or *periodical* disorders are only *suspended* by it.

The influence of specifics is on the principle of catalysis, and will consist of one of the following modes; the stimulation of special or particular glands to excrete or dislodge a deleterious something from the blood, by instilling their own poison, so to write, into the system, and thus expelling one more noxious or perilous, as iodine *versus* struma, arsenic *v.* lepriasis, and mercury *v.* syphilis; for sooner or later, if unduly exhibited, we must have iodism, arsenicism, and mercurialism; 2nd, the producing, on the principle of neutralization, some *tertium quid* as quiescent and innocent, as the product of an acid and an alkali. Such may be the effect of soda in excess of gastric acid, and the elimination of sulphuretted hydrogen by the combination of sulphur with hyper-hydrogenized blood; or a special controlling influence, as that of aconite and digitalis, on the pulse; or the subduing of hyperæsthesia or exalted sensibility, as the power of opium and prussic acid in neuralgia, or the imparting of the tone by bark in ague.

All this fully illustrates the truth of Hunter's axiom, and the two important practical secrets are probably these—to know when the disease is neutralised or dislodged, the moment for the suspension of our ministration, or the remedy will itself become disease—the antidote will prove a bane; and to apportion our doses to the idiosyncracy or systemic power of the patient, or we may salivate with a grain of calomel, or suspend or destroy the heart's action by almost homœopathic doses of laurel water. As an example of casual prophylaxis, I may glance at the influence of the chlorides on strumous diatheses. The factory girl, although hereditarily predisposed, when submitted to the inhalation of chloric atmosphere, will constantly resist the development of her taint of struma.

The object of this paper is of course limited, having reference chiefly to the lessons taught by the *vis medicatrix naturæ*—the removal of obstacles to her remedial progress, and the imitation of her salutary processes.

We see, then, that the two grand modes of antistasis which nature adopts to preserve and restore the health of the system are, elimination, and vicarious action. The former mode more especially prophylactic; the latter, curative. And I adopt this word antistasis, in contrast to that of metastasis, which merely implies the transmigration of the same especial action or quality from one locality or tissue to another, often of *more* importance, as the transference of rheumatism from the joints to the pericardium, or the fibrous tissues of the eye; whereas antistasis implies the development of spontaneous establishment of another action on a remote and usually *less* important tissue

in which that action was indigenous and hitherto latent; metastasis, moreover, being usually aggravating; antistasis, remedial.

Of course natural prophylactic elimination is constantly going on in the body; its integrity could not for a moment be evinced without it. Let it be checked or thwarted, and disorder in varied degrees instantly ensues. May not the loads of grape sugar in diabetes be a depuration and the prophylactic of disorder?

The pathology of the kidneys, ere the elaborate researches of Prout, Bright, and Bird, was often ignored, not only because its investigation was troublesome, but that the prominent symptoms were *elsewhere* apparent. Thus constantly arose internal metastatic action, vicarious deposits in the shut sacs of the body, ever perilous, often destructive, and the reflex contamination of the blood by the absorption of urea. This poisoning of the brain is noticed not only in absolute suppression of urine, but where, although in large quantity, it is of low specific gravity.

If the skin were in every point vicarious of the kidneys, it were well; but the transference is unhappily misplaced, the brain being often the scapegoat. In the comatose disorders of children we constantly see the value not only of urinary tests, but of ascertaining the condition of the renal functions.

I may venture to adduce two or three cases as indirect illustrations.

The daughter of a barrister, in Eccleston-square, had been indisposed for many days, the prominent symptoms being moaning, lopping or tossing of the head, frowning, coated tongue, and remittent febricula, so constantly marking these infantile maladies. The routine of grey powders, antimonials, and even powerful aperients, failed in giving relief. It was then ascertained that the urine had been extremely deficient in quantity, and diuretics, which speedily brought the kidneys into play, and the warm bath speedily relieved the head symptoms, and the child was convalescent. In retention of urine also I believe the coma and delirium so soon supervening arise from this poisoning of brain-blood, whether the mode be absorption or vicarious deposit, in consequence of non-separation by the renal gland.

I was called to Mr. P. His symptoms were headache, hot, dry skin, furred tongue, wandering, and clipping of the words. *Primâ facie*, it seemed that plethora was the sole cause, and I was about to adopt several modes of depletion. I discovered, however, that he had stricture, and, by cross-examination, that he had not passed urine for twenty-four hours. The passage of this was peremptory, but myself and my accomplished friend, Mr. H., failed in effecting this; and, as he resisted all our entreaties for an operation, he became more and more delirious, and rapidly sunk, a dissolution too rapid to be explained by the common course of typhoid fever. The bladder remained entire.

In July last, I was telegraphed to the further part of Surrey, to

visit a gentleman, whose perineum I was compelled to open very deeply, in consequence of an enormous gangrenous abscess, which was rapidly dissecting the perineal spaces. The symptom most distressing to the friends being muttering, acute delirium. The anatomy of the perineum was so completely deranged that it was with the utmost difficulty that I introduced a catheter into the bladder, and withdrew the urine of twenty-four hours' retention. In this case, within an hour the cerebral symptoms subsided—an amendment so rapid as to be inexplicable, except by the draining off of that which had by metastasis poisoned the circulating fluid, and restoring the function of subsequent elimination. On this principle, probably, we may explain the *tonic* effect of diuretics where the apathetic granular kidney has induced extreme asthenia and debility. How important, then, to assist the suppressed eliminations of nature! The skin is constantly vicarious of the kidney in profuse diaphoresis or sudamina—*i.e.*, as regards the mere *serum* or water; but it is not specially so, as I have observed, urea is not *excreted*. The renal apparatus seems essential for this elimination, although some medicines, essentially purgative, will, I believe, when the kidney fails, pass off through the skin with the profuse sweating. The derivative influence of the skin is also beautifully seen where extensive cutaneous vesications have, by a system of *compensation*, rapidly induced the absorption of hydrops pericardii. But it seems that no antistatic action can be induced to excrete urea; the only remedy is mechanical. Even the fluid of cholera—that specific fever so often fatal in the first stage—may be often antistatic or eliminatory, being but a profuse critical diaphoresis of the mucous tissue, as the sweating sickness was of the cutaneous pores; and this may have perhaps been the gist of Abernethy's system, the sweating of the membrane doing more than the stimulation of the liver by his pill. The salutary sequelæ of intestinal antistasis are daily witnessed. The happy result of critical or spontaneous diarrhœa was probably highly suggestive to Hippocrates and Sydenham, and the two modern oracles of alimentary elimination, Hamilton and Abernethy.

I believe that deficient defecation, also, is a constant source of this poisoning of the blood. In children who die of marasmus, we constantly see a pulpy mass, like decayed brown paper, quickly prone to septic changes, or acetous fermentation, closely coating the mucous membrane of the alimentary canal. Hence two morbid effects, negative and positive—a closing of the mouth of the lacteals, and an indirect contamination of the blood.

Habitual constipation, of young girls especially, lasting, as I have known it, for even twelve and fourteen days, I believe often proves a fertile source of their multiform and eccentric maladies, by absorption. This mode of endosmose is proved by the feculent odour imparted to pus in the vicinity of the lower bowel. No plan is more prejudicial than to treat this habitual constipation by habitual and drastic purgation. This mode is certain to increase the malady, which depends on

atonic state of the canal. Very minute doses of aloes, with soap, combined with enemata of warm soap-water, are far preferable; and this should be quickly followed by decided tonics, which will prove antistatic to this debility, and thus ensure healthy action. These are not the only cases maltreated on the routine of purgation, because we are fearful of the censure of blowing hot and cold. I see no incompatibility in the simultaneous exhibition of bark and foxglove. In habitually depraved secretions we again adopt the same error. It is not enough to dislodge the bad; we should ensure an improvement, both by the antistatic process of cutaneous activity and the healthy stimulation of the alimentary glands, by alterative tonics.

The more prominent mode in which the *vis medicatrix* is displayed is local or *external* antistasis—the establishment or development of morbid action in a remote organ, to relieve one primarily affected. This, perhaps, induced Sydenham to affirm "disease to be no more than a vigorous effort of nature to throw off the morbific matter, and so recover the patient." Boerhaave even wished to produce petechial fever as a counteraction to internal disorder. This may, of course, be perilous and fatal when a more important organ is secondarily attacked, as, for instance, the occasional transference of ulceration from an external burn to the mucous membrane of the duodenum. But nature is usually wiser than this, throwing out on the periphery of the body that which might destroy in the centre.

What a volume of cases might be cited of the salutary result of this spontaneous antistasis, and of its converse spontaneous recession, or what I have been often jeered at for terming unfortunate cures! We constantly boast of a victory that ought in strict justice to ruin us. The instances I may refer to are, milk fever and insanity, from lacteal suppression; meningitis, from suppression of favus; hæmoptysis and phthisis, from the stoppage of hemorrhages and fluxes; the development of tubercle, from amenorrhœa, &c.; and, indeed, the termination of pregnancy in parturition, the vicarious uterine action counteracting or suspending for a time the phthisical malady. How often, also, has accident indicated the mode we ought intentionally to have adopted. We have often known epilepsy, chorea, and other convulsive or spasmodic maladies relieved, nay, removed entirely, by an extensive burn or suppurating wound.

In the pathology of mania, especially, most emphatic lessons are written for us by accident, if we would but learn them. The history of the ravages which overworking commits in the highly intellectual brain teems with illustrations. I need only point to the case of the statesman and the philosopher, who, in what is termed temporary derangement, cut their throats. In the one, the physician who came only to see his lordship die, found that he had stuffed the wound with a towel in a moment of reason—the lucid interval consequent on the flowing of the brain-blood. In the other, the surgeon who stanched the wound was reprimanded by the patient for his neglect of that de-

pletion which he had urgently begged him to employ, and which he was certain would have entirely divested him of all attempt at suicide. The pathologist who reads the autobiography of Walford, in the July number of the Psychological Journal, will discover at once the source of the most distressing and awful train of monomaniacal illusions that ever excruciated an enlightened intellect. These are the progressive sequelæ of a *cranial contusion* in infancy; acute headache; coryza maligna; continued cerebral erethism; melancholy madness most protracted. And yet even Mr. Cline blinked the case by merely recommending fumigation; and Dr. Withering, yet more apathetic, adopted the *expectant* treatment. And what were the results of this sad trifling? Extensive dura matral adhesions. the membrane of *leathery* consistence; honeycomb of the inner table of the skull; cerebral *ramollissement* and *green* abscess, and extensive serous effusion. Which of us will not believe but that decided counteraction in this, as well as the other cases, might at once have relieved the symptoms, and snapped the chain of morbid changes that ended in so melancholy a catastrophe? Analogous cases might be cited: this one is sufficient for my purpose.

How beautifully does nature set up an antistatic action in cases of amenorrhœa! The sanguineous discharge from the ulcer, therefore termed the *menstrual sore*, is close on a compensation.

When a morbid idiosyncracy or diathesis exists, we may convert it into a most valuable mode of counteraction, both curative and prophylactic. Often in a fit, or a threatening of apoplexy, in a gouty diathesis, we should not hesitate, if we read nature's lessons wisely, to blister the foot with liquor vesicatorius, in order to solicit or induce an attack of gout.

In cases, also, where a wound assumes an unhealthy aspect, we should beat about to discover some habitual or constitutional malady, by the bringing out of which we may, by antistasis, remotely influence the wound. A few days after Sir Astley Cooper had dissected the cyst from the head of George IV., the lips of the wound gaped, and an ichorous discharge ensued. The accomplished surgeon, I believe, trembled for his patient, and even for the continued splendour of his own fame, when happily a desperate fit of gout supervened, and the king was saved. If Long, the Sangrado of endermic surgery, had known when to stop, or how to heal, I believe he might have had his tether to this day, unless we ourselves were for once wise enough to take a lesson in prophylactic surgery from a quack.

Those who have watched the cerebral disorders of children will remember how constantly symptoms have been aggravated by arrest of spontaneous antistasis, and how immediate is the influence of surface counteraction in cases marked with imminent peril, where time is not allowed for the effect of internal remedy. Indeed, if there be a class of patients especially obnoxious to physic, and yet are constantly overdrenched, it is the batch of babies, for whom the endermic method of

treating them is at our elbow. The infant of a well-known architect had taken a quarter of a grain of Dover's powder, which it is probable the chemist had carelessly triturated, when coma very speedily supervened. The pupil was contracted to a point, and the child seemed moribund. There was no time for drenching. The application of snow to the vertex caused the pupil instantly to expand, its contraction recurring as the snow was melting. Perseverance in this simple mode, however, ultimately relieved the brain of its congestion, and the infant recovered. I do not here enter on the jealousies between nerve and brain blood; I merely state the fact and the effect. If we do not all fully coincide in the aphorism of Hippocrates—"They in whom tumours and ulcers do appear are neither taken with convulsions nor driven into rage or madness,"—we must believe in the extreme value of superficial counteraction as a prophylaxis—a safety-valve in cases of tendency to, or even in, incipient disorders. Indeed, the latent or dormant germ about to be developed may be thus checked and blighted, or kept in abeyance, until by successive generations the systemic taint or tendency may at length cease to be hereditary. An issue has often checked hæmoptysis and tubercular growth, which might, in its default, have speedily lighted up consumption.

For the sake of brevity, I have not here discussed the comparative influence and value of *especial* counter-agents, and, indeed, I have merely jumbled together a tithe of what I might wish to record.—*Lancet, Dec.* 23, 1854, *p.* 526.

144.—ON THE MODES OF DEATH BY HANGING.

By Dr. J. S. Hughes, Examiner in Surgery to the Queen's University in Ireland.

The modes of death by hanging may be thus summed up, not in the order of their frequency, but of their fatality:—First, Luxation of the odontoid process of the second cervical vertebra, or fracture of the spine, and consequent injury to the medulla spinalis; Second,—Suffocation from pressure on the windpipe, producing complete obstruction to respiration; Third,—Congestion of the brain, or apoplexy from arrest of the return of blood from the head to the heart, the result of compression of the great bloodvessels of the neck; Fourth,—Injury inflicted by violent and prolonged pressure on the important nerves of the neck.

The first cause of death by hanging, or that by luxation or fracture of the cervical vertebræ, is rarely met with, even in those who have been criminally executed; thus, in four cases of this description, the late Dr. Houston found the "cervical verebræ ininjured, and also the spinal marrow and the brain;" and in the case of Hurley, who was hanged in August, 1853, for the murder of a young girl at Dunsandle wood, although the body had fallen from the drop, "with a

tremendous jerk, and oscillated for a few minutes," yet we are informed by Dr. Croker King, who made a minute examination of the culprit's body eighteen hours after the execution, that "there was no dislocation or fracture of the vertebral column or injury of the ligaments or spinal cord." Sir Benjamin Brodie states, that he has examined the bodies of several persons who had been hanged, and never found the spine to have been injured in a single instance.

It is now generally admitted that the most frequent immediate cause of death by hanging is that by suffocation, or exclusion of air from the lungs, the result of pressure of the rope or ligature on the windpipe, but in many cases death would appear to be produced by the combined influences of suffocation and apoplexy; indeed, according to Remer and Caspar, asphyxia and apoplexy operate in the greater number of cases of hanging as mixed causes of death. The rapidity of death by suffocation will depend on a variety of circumstances, which may be thus enumerated, viz.: the exact position of the rope or ligature round the neck, the weight of the body, the height of the fall, the age of the patient, and the quantity of air in the lungs at the moment the ligature was applied to the neck. The following are the physiological results which ensue soon after the respiration has been completely stopped. The heart, after a few beats, propels impure blood, that is, blood no longer oxygenated, to the brain, and, as the action of the heart and diaphragm depend on nervous energy, and as nervous energy requires a supply of pure or oxygenated blood, it follows that when the supply of pure arterial blood is cut off, the heart and diaphragm soon cease to act.

The heart continues to act for a brief period after respiration has completely ceased. When speaking of the rapidity of death in these cases, Dr. Taylor says: "Supposing the hanging to be unattended with violence to the parts about the neck, it is possible that some individuals might be resuscitated after five minutes' suspension, or longer; others again may not be recovered when cut down immediately after suspension, a fact which depends probably on the different degrees to which asphyxia or apoplexy have extended." Sir B. Brodie is of opinion that under ordinary circumstances it rarely happens that the heart continues to act for more than four minutes after the trachea has been completely obstructed; and he thinks in weak and exhausted animals the period to be shorter than in the strong and vigorous.

The great majority of physiologists are of opinion that once the heart's action has positively ceased, in consequence of the suspension of respiration, it cannot be renewed. Dr. Carpenter, however, says: "The cessation of the heart's action is due to two distinct causes, acting on the two sides, for on the right side it is the result of overdistension of the walls of the ventricle, owing to the accumulation of venous blood, and on the left to deficiency of the stimulus necessary to excite the movement. The property of contractility is not finally

lost nearly as soon as the movements cease, for the action of the right ventricle may be renewed for some time after it has ceased, by withdrawing a portion of its contents, either through the pulmonary artery, their natural channel, or more directly by an opening made in its own parietes, in the auricle, or in the jugular vein; or, on the other hand, the left ventricle may be again set in action by renewing its appropriate stimulus of arterial blood; hence, if the stoppage of the circulation has not been of too long a continuance, it may be renewed by artificial respiration, for the replacement of carbonic acid by oxygen in the air cells of the lungs restores the circulation through the capillaries, and thus, at the same time, relieves the distension of the right ventricle, and conveys to the left the due stimulus to its action."

Although in the third mode of death by hanging, or that by apoplexy, the sudden compression of the great bloodvessels of the neck cannot completely arrest the circulation of blood, inasmuch as it cannot reach the vertebral vessels; yet, if the pressure be sufficiently long and forcibly sustained, it can, by delaying the return of blood from the head to the heart, produce extreme congestion of the cerebral vascular system, which, as proved by post-mortem investigations, may, or may not be accompanied by extravasation. As, however, in a considerable number of the cases of attempted suicide by hanging, which have recovered, no paralysis ensued, we have some reason to conclude that in many cases the apoplexy which results from compression of the great bloodvessels of the neck is of the congestive form, a point of considerable practical importance, inasmuch as it is in that particular form of apoplexy that the voice of the profession is in favour of bloodletting.

Compression of the important nerves of the neck must necessarily be followed by injurious results, although it may not act as a proximate cause of death in these cases. Thus, J. J. suffered from considerable distress from laryngeal cough for several days after the attempt at suicide; he likewise complained of a loss of sensation in his jaws and ears, which were obviously ascribable to the forcible compression to which the nerves of the neck were subjected whilst the patient was in the state of suspension.

Much of the success of the treatment of a case of attempted suicide by hanging will depend on the nature of the injuries inflicted, the period at which assistance is procured, and a proper discrimination in the selection of remedial measures. Thus, if the surgeon be summoned to a person within a few minutes after he has suspended himself, although the breathing may have completely stopped, yet, provided there is neither dislocation nor fracture of the vertebræ, and consequent injury of the medulla spinalis, animation may, in many cases, be restored by an immediate recourse to artificial respiration. Electricity, or electro-galvanism, if within reach, ought at the same time to be applied to the cardiac and spinal regions; if, on the other hand, the patient is cut down before respiration has ceased, and

symptoms of congestion of the brain or apoplexy should present themselves, bloodletting ought at once to be resorted to. Although, as already stated, there is reason to believe that the apoplexy which results from compression of the great bloodvessels of the neck is frequently of the congestive form, yet, as we possess no infallible rule by which we can positively pronounce in these cases, whether cerebral hemorrhage has taken place or not, we should in every such case exercise as much caution in the abstraction of blood as we would in apoplexy from other causes; here, as elsewhere, we should, whilst endeavouring to remove congestion, or arrest hemorrhage, ever bear in mind that the brain has suffered from a violent shock, for the repair of which the system will require its best resources.

Much difference of opinion has existed both as to the quantity of blood to be taken away in cases of apoplexy, in which bleeding is indicated, and, as to the precise situation from which the blood should be abstracted. The condition of the pulse, and the effects of the withdrawal of blood on it, will be the safest guides as to the former; with regard to the latter, we generally select the temporal arteries, agreeing, as we do, with Abercrombie, who, whilst treating on the subject, says:—"Much importance has been attached to bleeding from the jugular vein, as more likely to give immediate relief to the head; but we must remember, that the only jugular vein that can be opened is the external jugular, which has little communication with the brain, and, consequently, bleeding from it is much inferior to bleeding from the temporal artery."—*Dub. Hosp. Gazette, Oct.* 1, 1854, *p.* 258.

145.—ON THE LAWS OF MORTALITY AND SICKNESS OF THE LABOURING CLASSES.

By Thomas Rowe Edmonds, Esq., Actuary to the Legal and General Life Assurance Society.

It has already been stated that sickness, in duration for each year of life, increases with the age, according to the same law which regulates the increasing proportion of deaths to the number living at successive ages. From the unity of law between sickness and death in relation to life, it necessarily follows that the sickness endured at any age bears a constant proportion to the deaths at any specified age. This proportion is commonly found to be, that of two years of sickness to one death. Occasionally this proportion is as low as one year and three quarters, and as high as two years and a half, of sickness to each death. In addition to the law determining the duration of sickness at each age, there is another law which connects together the number of attacks or cases of sickness with the number of deaths, and consequently with the number of living, and with the duration of sickness at each year of age. The law which regulates the attacks of sickness was first communicated to the public in 'The Lancet' of the

year 1836, (vol. i., p. 855,) in an article wherein I showed, from recent observations, that the fatality of cases or attacks of sickness increased with the age according to the law which regulates the increase of mortality and the increase of duration of sickness for each year of life. I then stated it to be an inevitable consequence of the identity of the numbers regulating the proportion of attacks of sickness to deaths, of duration of sickness to life, and of deaths to life at each age, that the number of annual attacks of sickness for a given number living was the same at all ages, and that the duration of each attack of sickness increased with the age in the same degree as the mortality increased.

The most interesting and remarkable of the above laws of sickness, is that which makes the proportion of annual attacks of sickness to the number living constant at all ages. Notwithstanding its extreme simplicity, the existence of such a law does not appear to have been suspected previous to its announcement; nor did there exist, at that time, any facts which could be appealed to as supplying direct evidence of its existence. The public were subsequently indebted to Dr. Farr for adducing facts directly proving the constancy of the proportion of the numbers attacked by sickness to the number living at various ages of adult life. In the returns from Friendly Societies published by Mr. Neison and Mr. Finlaison, a great multitude of facts have been collected in further corroboration of the proportion of attacks of sickness being constant, and independent of age—at least in the period of florescence. According to Mr. Neison, the constant proportion of living persons attacked by sickness, in any year between 20 and 50 years of age, is 22 per cent. According to Mr. Finlaison, the constant proportion is 24 per cent. in each year. In both observations there is a depression of about 2 per cent. in the attacks near the age of 33 years, which is the age at which the greatest number of admissions of new members occurs; and which consequently is the age at which "selection" might be expected to have the greatest disturbing effect, whether on sickness or on mortality.

In the period of senescence, or at ages exceeding 50 years, the facts bearing on the laws of sickness (whether of attacks or of duration) are everywhere imperfectly recorded, on account of the difficulty of distinguishing cases of sickness from cases of debility from old age. Judging from all observations hitherto made, there appears to be no ground for doubting that the law of attacks, like the law of duration, of sickness, is continuous throughout the two periods of "florescence" and "senescence," or from the earliest to the most advanced age of adult life. The returns of Mr. Neison are not available for the purpose of determining whether the proportion of attacks of sickness is constant at advanced ages, as well as at ages under 50 years, because no attempt has been made to distinguish therein sickness from old age. But the returns of Mr. Finlaison, which professedly represent pure sickness, free from cases of debility from old age, are sufficient for the present purpose. There is, however, a preliminary correction

to be made in the numbers which Mr. Finlaison takes to represent attacks of sickness, which correction is of small amount at ages below 50 years, but is of great magnitude at very advanced ages. Mr. Finlaison assumes the number of persons who have been on the sick-list in the course of the year to represent the number of new attacks of sickness in the year. This assumption is obviously incorrect, for in such list is included the cases of sickness remaining at the end of the previous year, and transferred to the new year. The number of such transferred old cases of sickness is represented by the proportion of living constantly sick at the particular year of age, which is the correction to be applied in diminution of Mr. Finlaison's alleged numbers of attacks. For example, according to Mr. Finlaison's tables, and taking decennial intervals of age, the proportions constantly sick at the ages 50 and 80 years, respectively, are $3\frac{1}{4}$ and 26 per cent. Also, according to the same tables, the annual proportion of persons on the sick-list is $26\frac{1}{4}$ per cent. at the age of 50 years, and 56 per cent. at the age of 80 years. If we subtract from the latter numbers the corrections previously given, we obtain the numbers 23 and 30 per cent. as the corrected per-centage of new annual attacks at the respective ages of 50 years and 80 years. These two numbers approach sufficiently near to each other to warrant the presumption that a defect in the observation is the cause of the failure to obtain an exact coincidence. The apparent excess of attacks at the age of 80 years is in all probability owing to the complication of debility from old age with cases of sickness. Seven out of 30 cases of sickness would not have had the visible effect of producing incapacity for labour, if the man of 80 had not been subject to a debility from which the man aged 50 was comparatively free.

On inspection of the Table, it will be seen that at the mean age of 40 years, the rate of sickness obtained by combining all three observations, is 1·17 weeks to one year (or 52 weeks) of life, which is the same thing as 2·24 years of sickness to 100 years of life, or 2·24 per cent. of the living constantly sick. From the same table it will be seen that the mean number of deaths at the same age, according to the same combined observations, was 1·02 in one year out of every 100 people living. There was consequently 2·24 years of sickness to 1·02 deaths, or 2·20 years of sickness to one death. This relation of sickness to death remains constant when other ages are compared in the assumed observation representing the mean of all three observations.

The relation of sickness to death, which is constant for different ages in the same observation, is subject to variation in different observations. For example, in rural districts, the proportion of sickness to one death, at every age, is found to be 2·52 years, when the average of all three observers is taken; whilst in town and city districts, the averages similarly obtained are 2·29 and 1·92 years of sickness to one death. This result, from combining the three observations on each of the three districts, is not, however, in accord with the separate obser-

TABLE.

SHOWING, *for Decennial Intervals of Age, the Rates of* SICKNESS *in Rural, Town, and City Districts, according to each of three Observers; also showing the Mean Rates of Sickness and Mortality in each of the same three Districts, for comparison with the results of Theoretical Tables, in all of which the period of "Senescence" commences at the age of* 48 *years.*

AGE.	RURAL DISTRICTS.			TOWN DISTRICTS.			CITY DISTRICTS.			MEAN OF 3 OBSERVATIONS.		
	Finlaison.	Neison.	Ratcliffe.	Finlaison.	Neison.	Ratcliffe.	Finlaison.	Neison.	Ratcliffe.	Rural.	Town.	City.
	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.	Weeks.
25 to 35	1·02	·87	·78	·96	·89	·83	·90	1·11	·84	·89	·89	·95
35 — 45	1·19	1·08	·99	1·18	1·30	1·08	1·18	1·50	1·28	1·09	1·19	1·32
45 — 55	1·61	1·65	1·76	1·79	2·62	1·80	1·68	2·46	2·26	1·67	2·07	2·13
55 — 65	2·78	4·08	4·54	3·00	5·17	4·10	2·63	4·63	5·58	3·80	4·09	4·28

TABLE.—(*Continued.*)

AGE.	*Sickness* Observed. Mean of Three Observations of all Three Districts.	*Sickness*, Theoretical, Corresponding to Mean of Three Observations.			*Mortality* Observed, being Mean of the Three Observations.				*Mortality*. Theoretical. Corresponding to Mean of Three Observations.			
		Rural.	Town.	City.	Rural.	Town.	City.	Three Districts.	Rural	Town.	City.	Three Districts.
	Weeks.	Weeks.	Weeks.	Weeks.								
25 to 35	·90	·84	·92	1·02	·73	·77	·99	·80	·64	·77	1·02	·77
35 — 45	1·17	1·13	1·24	1·37	·86	1·04	1·45	1·02	·86	1·03	1·37	1·03
45 — 55	1·87	1·73	1·88	2·09	1·35	1·68	2·18	1·61	1·31	1·57	2·09	1·57
55 — 65	3·95	3·65	3·98	4·43	2·72	3·45	3·12	3·01	2·77	3·32	4·43	3·32

vations, for Mr. Finlaison states the disproportion between rural and city sickness to one death to be much greater than the disproportion stated by the other two observers.

The present three observations, when closely investigated, do not, however, either of them make good the proposition, that the quantity of true sickness to one death is less in cities than it is in rural districts. Mr. Finlaison, in showing the effect of "heavy" labour in increasing the apparent sickness, has supplied a cause sufficient to account for the apparent excess of rural sickness over town and city sickness. The apparent sickness, according to Mr. Finlaison, depends in a great measure on the occupation or labour of members being "heavy" or "light." He has shown that, at any given age, whether in rural or city districts, whether the mortality is high or low, the apparent sickness is 20 per cent. greater for heavy than it is for light labour. In the two cases, the real amount of sickness is probably the same, but the incapacity for labour, or apparent sickness, is 30 per cent. greater in the case of heavy labour. This excess probably arises from minor degrees of sickness, which are sufficient to incapacitate for heavy, but not for light, labour. The proportion of "heavy" is much greater in rural than in city districts. We have thus a reason for expecting more apparent sickness in the former than in the latter districts. The cause now mentioned, when taken in combination with the opposite methods of treating superannuation, is also sufficient to account for the disproportion of rural to city sickness being greater, according to Mr. Finlaison, than it is according to the other two observers; for a considerable proportion (one-fourth part) of the sickness exhibited in the tables of Mr. Neison and Mr. Ratcliffe, consists of the life-time of superannuated members, who do not engage in labour, whether heavy or light. On the other hand, Mr. Finlaison, in his tables, takes no account of superannuation, except to exclude it from any connexion with sickness. It thus happens that all Mr. Finlaison's recorded sickness is affected by the distinction of heavy and light labour, whilst three-fourths only of the sickness recorded by Mr. Neison and Mr. Ratcliffe is so affected.

The laws of mortality, of sickness, and of health, may be said to be the three chief regulators of human life. It has been already shown, that there exists such a parallelism between the two laws of mortality and sickness (in duration,) that when the mortality at any age is known, the sickness at the same age is also known, by means of the existing constant relation of sickness to death at all ages, (about two years of sickness to one death.) It remains to be seen that the degrees of health at different ages also bear a simple and constant relation to the mortality. It is first to be noticed that health-time, being that portion of a man's life-time which is not sick-time, will of course decrease with the age as sick-time increases; so that, at very advanced ages, sick-time and health-time will form nearly equal portions of a man's life-time. At the early ages, however, of adult life, the proportions of

health-time to life-time decrease so slowly, that, for the purpose of estimating the amount of health enjoyed at any age between 20 and 50 years, the element of time may with propriety be neglected, and the degree or intensity of health be alone regarded. On reflection it will be easily perceived that the degree of health possessed at any age is identical or synonymous with the vital force possessed at that age. The mortality is the inverse measure of the vital force, and is consequently the inverse measure also of the intensity of health.

For the purpose of illustrating the combined effects of the laws of mortality, sickness, and health, let any two ages in the period of "florescence," differing from each other by the interval of 24 years, be taken, this being the interval in which mortality doubles itself. For example: if the mortality, sickness, or health, be numerically expressed for the age 20 years, we have only to multiply such mortality or such sickness by 2, in order to obtain the numbers expressing the mortality and sickness respectively as existing at the age 44 years. Similarly, the intensity of health at the age 44 years, is obtained by dividing by 2 the number expressing intensity of health enjoyed at the age of 20 years. If, instead of comparing people of different ages in the same class of population, we compare people of the same age in different classes of population, whereof the mortalities differ in the proportion of 1 to 2, we obtain the same results as those just stated: if the mortality is increased 100 per cent., the sickness will be simultaneously increased 100 per cent., and the intensity of health reduced 50 per cent. Sickness and death are unquestionable evils, which increase as the force of life or health decreases. Additions to intensity of life or health are unquestionable benefits, because they have the effect of diminishing unquestionable evils. Independently of these unquestionably beneficial effects, there is ground for assuming that life or health is intrinsically a benefit: whence it would follow, that additions to intensity of life, or robustness of health, must also be benefits. If the intensity of life or health be doubled, there is nothing to oppose the conclusion that the intrinsic benefit, pleasure, or happiness is also doubled.

All three observers have devoted much of their attention to investigations of the mortality of members of Friendly Societies, classed according to occupation, with a view to discover the effect of any particular branch of labour or occupation in prolonging or shortening the duration of life. So far as regards the great majority of branches of labour examined, the comparative results elicited by the three observers are unsatisfactory, since they fail to exhibit any principle of classification applicable to trades in general. In one important respect, however, the three observers are nearly agreed, viz., as to the two classes which occupy the extremes of the scale of vitality. The highest rank is occupied by agricultural labourers; the lowest rank is occupied by those in the occupation of clerks. The highest degree of vitality belongs to the class whose corporeal powers are most ex-

ercised; the lowest degree of vitality belongs to the class whose corporeal powers are least exercised. According to the observations of both Mr. Neison and Mr. Ratcliffe, the vitality of clerks is lower than that of painters, potters, and miners; that is to say, an occupation involving no corporeal labour is apparently more detrimental to life than occupations involving corporeal labour in a poisoned atmosphere.

According to the observation of Mr. Finlaison, "heavy" labour in rural districts is attended by increased vitality, whilst in town and city districts heavy labour has no similar advantage over light labour. This opposition of results is open to question on the ground of imperfect classification of the trades taken as the constituents of heavy labour and light labour respectively. A large proportion of the trades carried on in towns and cities have been classed by Mr. Finlaison as heavy labour, although confined to the hands and arms, and involving little exercise of the chief or lower limbs of the body. Such labour cannot, with reason, be considered as the counterpart of the unmistakably "heavy" labour of the agriculturist. It is highly probable that, if the distinction between heavy labour and light labour were as well marked in towns and cities as it is in rural districts, a similar advantage would be shown to exist in all three cases in favour of the vitality of the classes whose labour is "heavy."

The amount of bodily labour undergone, or muscular force exerted, by any particular class of labourers, appears to afford a correct index of the vitality of that class. This principle, which has been shown to be applicable to the opposite extremes (agriculturists and clerks) in the scale of vitality, would probably be found applicable to the intermediate classes, if they could be arranged in well-marked grades of labour undergone. This principle, when applied to the aggregate of trades collected in rural, town, and city districts respectively, will serve to explain why the vitality of rural is greater than that of town populations, and that of town greater than the vitality of city populations. For it cannot be doubted that the average amount of labour undergone by each man is greater in rural than in town, and in town greater than in city, districts. The disparity in the vitalities of the rural, town, and city populations would be greater than that observed if the native residents alone of rural, town, and city districts had been compared; for there is a great interchange of population by migration between rural and town districts, and between rural and city districts, whilst at the same time the balance of gain by migration is always against rural districts, and in favour of town and city districts. This transfusion of the rural element unduly elevates the vitality belonging to the native populations of towns and cities, at the expense of the vitality belonging to the natives of rural districts. If such recruiting from rural districts were stopped, the population of a large city, in however healthy a locality, would, in no long period of time, become unable to maintain its members by propagation.

When classes of people engaged in different occupations are com-

pared, there appears to be no reason for doubting that the vitality of each class is proportional to the amount of labour performed by that class. It is only when single individuals of the same or different classes are compared that the principle is found to be inapplicable. The vitality of an average agricultural labourer is greatly superior to that of an average clerk; but there are many clerks who possess a greater degree of vitality than that of an average agricultural labourer. An agricultural labourer commonly exercises his power of labour to its full extent, whilst many a clerk, possessing a great power of labour, does not exercise that power. In the former case, labour undergone and capacity for labour are almost exact measures of one another; but in the latter case the labour of an individual is no index of his capacity for labour. Amongst individuals engaged in the same occupation, it may be said, with great appearance of truth, that the vital force of each is proportional to his capacity for labour. Having arrived at this principle, governing the vitality of the members of any one trade, there appears to be no obstacle to the extension of the principle to individuals of different trades when compared together, and to say generally, that the vitality of any individual is proportional to his capacity for labour. In the majority of occupations (excluding that of agricultural labourers) the capacity for labour is generally in excess of the labour actually performed; but the proportional excess is probably nearly the same in most trades, whilst the excess itself has a continual tendency to diminution. This tendency is a consequence of the general law, that capacity for labour increases with the full exercise of that capacity, but diminishes when that capacity is insufficiently exercised.

The vital force of any human being is closely and almost inseparably connected with the capacity for muscular exertion. The greater the muscular force, the greater the vital force; a conclusion that might be arrived at without the aid of statistical observations of the number living and dying. Nearly all the known elements of animal vitality are included or involved in the capacity for hard labour. The animal organisation is tasked to its full extent by hard labour; if any of the principal organs be unsound, hard labour will prove the defect. The capacity for long continued hard labour cannot exist without powerful digestive and respiratory organs, a great strength of muscular fibre, and nearly all other known elements of strong vitality.

The vital force of mankind (and probably of animals in general) is subject to a composite law, consisting of three parts, one of which is immutable, and the other two variable, and capable of being influenced by human agency. The constant annual rate of increase of the vital force throughout the period of "childhood," the constant annual rate of decay of the vital force during the period of "florescence" or reproduction, and the greater constant annual rate of decay of the vital force during the period of "senescence," are in all probability immutable quantities which, like the law of gravitation, form part of the

foundations of the universe. The second part of the composite law consists in the extent of the two periods of childhood and florescence respectively. In different masses of population, and in the same mass at different times, the extent of the period of childhood has been known to vary so much as three years, and the extent of the period of florescence so much as ten years. The cause of these variations is yet unknown. If the cause, when discovered, should prove to be under the control of human agency, the extent of these periods may be permanently prolonged, of which the effect would be more than a proportional prolongation of human life. The third and last part of the composite law of vital force, consists in the absolute amount of that force possessed at birth, or at any specified age, by the average individual of a class. This amount in England has been shown to vary so greatly, that there exist classes of population whose mortalities at the same age differ from each other in the high proportion of one to three. These great variations have been shown to have a close connexion with degrees of bodily labour, and are apparently, to a great extent, under the control of human agency. Without any interference of man, the laws of nature operate to promote the increase of the classes possessing the higher degree of vitality, and to retard the increase of the classes possessing the lower degrees of vitality. It is, however, within the power of man greatly to aid the operations of nature, by offering additional rewards to the higher degrees of labour, and affording additional facilities for the increase of that part of the population which is now ascertained to possess the highest degree of vitality.—*Lancet, Dec.* 2, 1854, *p.* 453.

146.—ON SOME USES OF CHARCOAL IN SURGERY.

By Lawrence Ormerod, Esq., resident Medical Officer to St. Mary's Hospital.

[Great interest has lately been excited by the effects of charcoal as a deodorising and disinfecting agent. Amongst other good effects we shall probably find that it will prevent an attack of epidemic gangrene and even hasten the cure of one so attacked. Mr. Ormerod says,]

I took the body of an English terrier (weight about 10 lbs.), placed it on a stone floor in a small apartment, and lightly covered it with charcoal. Although the weather was very warm, and the body in a state of the most utter putrefaction, not the slightest odour could be detected. By some accident the charcoal was disturbed, and a large portion of the mass was left uncovered. In spite of this, the circumjacent charcoal was sufficient to prevent any offensive stench. Upon seeing this, I left the body completely uncovered, merely surrounding it with the deodorising agent. This, again, prevented any disagreeable smell. Having determined this fact, I again covered the carcase. Dr. Stenhouse, in his paper above mentioned, remarks, that charcoal was

not, as generally believed, an antiseptic, but, on the contrary, acted most powerfully as a hastener of decomposition. This statement was likewise fully corroborated by my experiment, as in less than a fortnight not a particle of flesh remained upon the bones, which were picked perfectly clean, and were of a snowy whiteness. I would in parenthesis state, that the plan I have mentioned would admirably suit the purpose of those who are in the habit of preparing skeletons for the use of scientific museums. The weight of the charcoal used in this experiment was about 5 lbs.

The fact, that the mere propinquity of charcoal to decaying animal matter is sufficient to counteract the horrible tendency that exists in such matter, is one of great importance; and the application of it to practical purposes must be of much utility. Why should it not now be used in the Hospitals for our brave army in the East? In spite of all the care that may be taken to render a military institution of this kind cleanly and sweet, still the difficulty of doing so is great in the time of war. A battle, or even a skirmish, sends in suddenly a troop of wounded. All immediately is hurry and confusion; all hands are called upon to relieve the urgent necessities of the sick; the wards become filthy, because necessarily neglected; a stench arises, not only from the bodies of the crowded soldiers, but from decaying animal matter, the proceeds of wounds and ulcers; then that terror of the surgeon, hospital gangrene, arises, laughing to scorn all the best efforts of science and humanity to check its course. What, then, more beneficial could be afforded than a substance that possesses the marvellous property, not only of destroying an odour most distressing to the patient, nurse, and doctor, but also of destroying the malignant influence of the organic matter from which such a noisome smell arises, reducing putrefying matter to innoxious compounds, so that, with a rapidity most remarkable, it can even take up dead flesh from bones without either visible or palpable action? I understand that extensive use is now being made at St. Bartholomew's Hosiptal of charcoal in the dissecting rooms. It is intended to use bolsters and mattrasses filled with it in cases of infectious disorders. If, in that great institution, proverbial for its cleanliness, for its salubrity, for its freedom from taint, and absence from all contagious disorder resulting from filth and malarious atmosphere, such measures are thought necessary to be adopted for the benefit of its inmates, surely some like means might be taken in the military hospitals, unhappily, although perhaps unavoidably, the seat of contagion, dirt, and inconvenience. Dr. Stenhouse having, however, fully pointed out, in his letter to the 'Times,' the advantages to be derived from this use of charcoal, I will leave that branch of the subject, and proceed at once to the question as to its value in a remedial point of view; merely observing, that, as reported in the papers, hospital gangrene has already broken out with virulence in Scutari, and instant measures should be adopted to use all plans which science and practical experiment have shown to be efficacious.

In the treatment of gangrene, the surgeon has three objects in view: the first is constitutional support; the second is the quick removal of the slough; the third is the destruction of that malarious influence which causes the disease to spread from one wound in a ward to all wounds that may be in it. The first of these three is foreign to the present object of this paper. To the two last, I now beg to direct attention.

1. The slough is to be removed by means partly local and partly systemic; and it is of the utmost importance that it should be removed as soon as possible. Not only does its presence act as a drain upon the body, weakening the patient by directing all his vital energy to the casting off of that which has become a foreign substance, but, every moment that it remains, there is a chance of absorption of the putrid matter into the system,—the almost sure precursor of death. The formation of the line of demarcation depends, of course, upon vital action, and local applications are of very little, if of any use, to hasten its appearance. The constitution must be supported well by stimulants and nutrients, in order to be able to perform the extra duty imposed upon it. But every means should be taken to remove rapidly the portion already dead, and cumbering the subjacent living structure with its baneful presence. The mechanical means adopted to this end are awkward to the surgeon, and unpleasant to the patient. Clipping away portions of the slough, as fast as it is formed, by means of a pair of forceps and a pair of scissors, is an operation of no small severity, if we can judge by the exclamations of the sufferer. The part affected is intensely irritable; and, however carefully and humanely such removal may be attempted, there must invariably occur sundry pulls and twitchings upon the highly sensitive living part beneath. It is, indeed, rare that such an attempt is not accompanied by a greater or less amount of bleeding,—a proof that the living part has been wounded. Moreover, such removal is meant to be merely partial, and for the sake of convenience; it is not done with a therapeutical intention. If, therefore, a substance could be furnished that would rapidly absorb the slough, as soon as it was formed, without pain, without inconvenience, easy in application, positive in result, it will at once be conceded that a great disideratum has been obtained.

The cataplasma carbonis of the Pharmacopœia Londinensis has been long and extensively used in gangrene and fetid ulcerations. Its formula is:—

℞. Aquæ ferventis fl. ℥x., panis ℥ij., seminis contriti lini ℥x., carbonis contriti ℥iij. M.

Owing, however, to the expense of pure carbon, peat, or better still, bone charcoal, is most generally used; but, in consequence of the impurities contained in the two latter, the bulk of the poultice is made of that substance, linseed-meal being mixed up with it. Charcoal having been long known as an efficient deodoriser, this cataplasm has

been a favourite remedy for stinking wounds; but I am not aware that any notice has yet been taken of it as an external therapeutical agent; it has been considered merely as a preventive of noisome smells, not as an able assistant in the cure of disease. Even so acute an observer as Dr. Pereira observes, in his great work:—"In this country it is used as a therapeutic agent, principally as a disinfectant and antiseptic, to absorb the fetid odour evolved by gangrenous and phagedænic ulcers. For this purpose it may be used in the form of powder or of poultice. Its disinfecting and deodorising powers, however, are much inferior to those of chlorine or of the chlorides of lime and soda." Now, it has been proved that charcoal is the very reverse of an antiseptic, and that its vast power of hastening decomposition is the cause of its utility, not only as a deodoriser, but, as I hope shortly to prove, as a local therapeutical agent.

Exception must be taken to the formula of the prescription above written. Although charcoal does not entirely lose it power when saturated with water, still its efficacy is much diminished. And this assertion is not weakened by the fact, that it is often thrown into the water in casks in vessels about to take long voyages, to "keep it sweet," as it is said. Sufficient charcoal is afforded to allow all bad matter to be absorbed. A case can be mentioned in point: in this hospital, charcoal was placed, in order to catch the urine dribbling from a patient suffering from incontinence, and so prevent the stench. When the charcoal was soaked, the smell was as bad as ever. The drier the charcoal, the more easily do the odoriferent particles enter its pores and become innoxious. It is remarked, in the notices of Hospital Therapeutics published in the number of this journal of Dec. 16th, that one of the necessary conditions that the fetor should be destroyed, is the perfect dryness of the article.

Remembering, therefore, the foregoing conclusions, which, for convenience sake, I now reiterate—viz., that it is highly important that the slough should be removed as soon as possible; that all mechanical means used to remove it are incomplete; that, as proved in my experiment upon the dog, charcoal is a wondrous hastener of decomposition; and that, for the perfect performance of this duty, it should be dry; it was determined to alter the usual formula of the poultice, in order that its effects might be seen upon the slough. The linseed-meal and bread were omitted, and as little water as possible was added—in fact, just enough to cause the poultice to cohere until it was placed upon the body of the patient, the natural heat of which soon caused the evaporation of the moisture, so that the application in a very short time was perfectly, or very nearly, dry. This plan was adopted in order to prevent the mess and consequent annoyance which would have been caused if the wound had been merely sprinkled over with powdered charcoal. There is some difficulty in concocting such a poultice as I have described; so I have thought of remedying this by having it made wet, and then putting it before the fire until nearly all the water is evaporated. This latter remark

may appear trivial; but if I am fortunate enough to influence some hospital surgeons to use this poultice, I shall be satisfied if it prevent a groan and a mental curse from the nurse who has to make it. Both peat and some charcoal have been used; of the two, the latter is preferred, but I am not aware that it is much superior to the former in its action.

Poultices made in this fashion have had a most surprising effect in rapidly absorbing sloughs when fairly formed and (so to speak) isolated. They act upon the dead flesh of the wound as the powdered charcoal acted upon the dead flesh of the dog. In St. Mary's Hospital last summer, there occurred an attack of epidemic gangrene. In one case it appeared, in a most severe and aggravated form, in the stump of a boy who had had his hand removed by Mr. Coulson, the sloughing extending up the arm for about two inches. In three days the slough was removed entirely. In a neighbouring patient, who had suffered the like operation followed by the same misfortune, and treated with linseed-meal poultices, and the unguentum elemi, the slough took a week and more to remove. I enquired just now of one of our most intelligent "sisters," "What is the difference between the time of removal of a slough if treated by the charcoal poultices and if treated by the 'green dressing' and linseed-meal?" Her answer was, that, by the former plan, it was taken away in half the time. An infant, suffering from a severe burn, had two enormous sloughing masses, extending, in one leg, from the knee to the ankle; in the other, involving the whole of the calf. In two days and a-half both were removed. A man was admitted, Dec. 15th, with a sloughing sore upon his leg. A poultice of this kind was put on. In six hours a most decided change had appeared in the sore; the dead portion was reduced in size fully one quarter.

Such, then, is the power of this substance; its claim to it is easily verified by simple experiment.

Thus is the desideratum obtained; a substance, well known, inocuous and inodorous, is found, *which rapidly absorbs the slough as soon as it is formed, without pain, without inconvenience; easy in application, positive in result.*

2. The deodorising and disinfecting power of charcoal having been well tested by experiment, little remains to be said concerning the other division of the subject to which I wish to draw attention, viz., the possibility of preventing the malarious influence causing the disease to spread from one to all wounds in a ward.

It is not my intention to enter upon the subject of how does this epidemic spread in hospitals. Suffice it to say, that actual contact extends the mischief, and that often a patient lying near another suffering from it contracts the complaint. The distressing smell is alone a proof that particles of decomposing animal matter are flying about. Charcoal has most indubitably the power of absorbing these; the dryer it is, as I before remarked, the easier is its task. The charcoal poul-

tice, then, made as I have described, will effectually prevent any odour or any putrefying portion proceeding from the slough and pervading the apartment.

I wish to lay no claim to ought of originality in this paper. If there is truth in my statements, the whole merits is due to Dr. Stenhouse, whose arguments arrested my attention, and to whose communication to the Society of Arts I am indebted for the idea.—*Med. Times and Gazette, Jan.* 13, 1855, *p.* 28.

147.—ON CHARCOAL AS A DISINFECTANT.

By G. J. Barford, Esq., St. Bartholomew's Hospital.

The substances with which we are acquainted as disinfectants or deodorizers are, chlorine, chloride of lime, lime, charcoal, &c., each of which possess this power to a certain degree, but not all acting in the same manner. The disinfecting power of chlorine depends on its affinity for hydrogen; thus, decomposing water or aqueous vapour, by uniting with the hydrogen, while the nascent oxygen oxidizes the organic matter, so that unless aqueous vapour is present, chlorine loses a great part of its disinfecting powers, and simply disguises the noxious effluvia, and is itself an irritating, offensive, and corrosive substance. Chloride of lime acts by the oxidation of the putrescent matter; but to do this effectually it requires the presence of an acid; thus, unless a considerable quantity of carbonic acid is present to decompose the hypochlorite of lime, and give rise to the evolution of hypochlorous acid, the chloride of lime will do but little as a disinfectant. Lime acts by the absorption of carbonic acid and sulphuretted hydrogen, leaving other noxious gases unaltered; thus they are all open to serious objections; but the one which practically will be found the most effectual, I believe, has received the least patronage—this is *charcoal*, a body whose disinfecting powers has long been known, but its mode of application has been quite neglected.

Dr. Stenhouse lately called attention to his very ingenious ori-nasal respirator, which depends on charcoal for its efficacy, the action of which is given in the 'Journal of the Society of Arts' for Feb. 1854; the respirator having been noticed in 'The Lancet' of Nov. 25th, I need only mention it as an instance of the powerful disinfecting power of charcoal; but at once call attention to the plan I have adopted in the application of this agent as a disinfectant, bearing in mind the results of Dr. Stenhouse's experiments, which prove that charcoal not only absorbs noxious vapours and putrescent odours, but at the same time oxidizes them; or, in other words, makes them undergo a slow but sure combustion, which must have its end in the conversion of deleterious gases into compounds, whose physical and chemical properties would admit of an easy separation or removal from their bed of formation, and which on evolution would not be the least deleterious. I therefore, previous to its use, heated the charcoal thoroughly

in a covered crucible, with a small hole in its lid, to allow any oxidized material which it might contain to escape, taking care not to have the hole sufficiently large to allow the charcoal to undergo combustion; when thoroughly heated it was allowed to cool, so that on exposure to the air it should not oxidize; in this state it was put into shallow vessels, and placed wherever putrescent odours existed, and in a few minutes the whole of the smell disappeared; but in a day or two the charcoal lost its power. I then thoroughly heated it again, with the same precautions as before, and placed it to perform its duties a second time, which it did with as much efficacy as on the first application; thus, by the repeated cleansing of the charcoal every or every other day, it does not deteriorate, but the same quantity will effectually remove noxious gases for an indefinite period of time.

With Mr. Holden's permission I was enabled to give it a most perfect trial in the dissecting-rooms of St. Bartholomew's Hospital, which at this time of the year must abound in noxious gases and putrescent odours, thoroughly heating the charcoal, and placing it in shallow vessels about the rooms. It acted so promptly, that in ten minutes not the least diffused smell could be detected. So quick and effectual was its action, that arrangements are being made for its constant use. It answers just as well as a purifier of water-closets, drains, wards of hospitals, and sick rooms. As a purifier of hospital wards, both civil and military, it might be applied with great advantage, saving patients from the unpleasant smells and effluvia from gangrenous sores, and for this purpose a wire gauze construction, containing the charcoal, might be made to surround the affected part at some distance from the dressing; thus the patient himself, and those in adjacent beds, would not be subjected to the influence of the putrescent odours. All these the charcoal would effectually absorb, doubtless with advantage to the patient and his neighbours also. Other quantities of charcoal might be placed in shallow vessels about the wards, and purified every morning as above mentioned. Being at the command of the poor as well as the rich, it admits of universal use; and though it may be objected to as a purifier of the wards of hospitals and chambers of the sick, under the fallacious notion that it would emit carbonic acid, and also on undergoing its daily cleansing would again give off the absorbed gases, yet this notion can never enter the minds of those who understand its action, seeing that carbonic acid cannot be generated unless the charcoal is heated in free contact with the air. This is prevented by having a covered crucible, in which it can be heated to any temperature without undergoing combustion; and the supposition that the absorbed gases are given off again when the charcoal is heated, will be removed by the fact, that they are all oxidized and converted into sulphuric, nitric, carbonic acids, and water, &c., and the heating of the charcoal is for the whole and sole purpose of removing these bodies, which exist in so small a quantity, that they could not be the least prejudicial, even if driven off in the centre of an inhabited room,

but, of course, they all pass up the chimney. Thus charcoal is more efficacious than any other disinfectant, when applied as above described, absorbing gases of whatsoever kind, not requiring the presence of any other substance to resist its action, but without stint or scruple collecting noxious vapours from every source, not disguising, but condensing and oxidizing the most offensive gases and poisonous effluvia, converting them into simple, inert, stable compounds; it is simple and economical, coming within the reach of the poorest, and can safely be placed in the hands of the most ignorant, thus combining advantages not possessed by any other disinfectant.—*Lancet, Dec.* 16, 1854, *p.* 505.

It seems almost impossible to exaggerate the importance of the recent modes which have been devised for the application of charcoal to disinfecting purposes. It is still used with the greatest success in the St. Bartholomew's dissecting-room. A few pans of charcoal, thinly spread, are exposed in various parts of the room, and all the trouble needed is, that the material be kept dry. For this it is necessary simply to warm it before a fire or on a stove once daily. Its effect on the atmosphere of the rooms becomes apparent in a very short time indeed after its introduction, and the place is constantly preserved with an odour not more offensive than that of a cleanly-kept butcher's shop. The advantage is not only in respect to smell, since it is further observed, that the bodies keep much longer and in better condition,—a fact easy of explanation by reference to the influence of a noxious gas-laden atmosphere in promoting decomposition. The grand recommendation of the charcoal is, that it is at once an effectual and a *bona-fide* disinfectant. It does not conceal the odours, or substitute others for them; but actually destroys, by decomposition, the poisonous materials on which they depend, and replaces them by others, either innocent, or even useful. Its action, as is well known, is by mechanical catalysis.

The applications to which charcoal may thus be put seem to be quite without limit. Here is a substance by which, almost without trouble or expense, an end may be put to the odours of the dissecting room, the ward, or the sick chamber. The smell of the cesspool, the sewer, or the ill-constructed water-closet need no longer be tolerated. The candle-maker, the soap-boiler, the tanner, and the bone-grinder, may henceforth pursue their respective avocations without being nuisances to the neighbourhood. It was supposed, at first, that it would be necessary to compel any noxious gas to pass through the charcoal as through a sieve, in order to secure its decomposition, and on this supposition depended the principle of the "charcoal coverlets" for wounds which we described some months ago. It is now found, however, that no such necessity exists, inasmuch as the substance preys with such avidity on the pernicious elements that it requires only to be exposed to free communication with them. Thus, to deodorise a Hospital ward, it is only necessary to place plates of dry charcoal at

short intervals about the room, say one to each bed, and to secure them from moisture. If only one case in the ward be the cause of smell, it may suffice to hang from the bed of that patient a muslin bag, containing the invaluable material, or to lay it anywhere in near proximity.

A man is now attending the Hospital for Diseases of Skin on account of syhilitic rupia, who has lost his nose, and suffers from caries of the nasal bones. It is proposed to have constructed for him a gutta percha nose, and to place within it a receptacle for charcoal, by which means he will be enabled to avoid being, as he has been for years, a nuisance to all who approach him.—*Med. Times and Gazette, Feb.* 10, 1855, *p.* 136.

148.—SOME REMARKS ON CHLOROFORM.

By Dr. Corrigan, Physician in Ordinary to the Queen in Ireland.

[Dr. Corrigan makes the following remarks on the exhibition of chloroform by the stomach. He says,]

First—With regard to its dose. For a considerable time past we have been in the habit in our hospitals of administering chloroform in larger doses than are usually advised in works on Materia Medica. To procure sleep in fever (*vid.* Dr. Gordon's instructive communications in Hospital Gazette of February 1st); to afford relief from pain in painters' colic, &c., the minimum dose given is usually half a drachm repeated every two or three hours, and the dose is very frequently increased to a drachm. In delirium tremens, the dose is generally a drachm at first, and is often increased to two drachms. It may, however, be taken not only with impunity, but with advantage in much larger doses.

On the 18th February, I saw Mr. H——g. He had been leading a most intemperate life, drinking as much as two pints of whiskey daily for a fortnight previously.

When I saw him, he was without sleep for the fortnight, and with the usual signs of the onset of a most severe attack of delirium tremens. I prescribed for him the following mixture:—

Chloroform, ℥v., pulv. glycirrhiz. ℥ v., mist. camph. ℥ixss.

with directions that he was to take at a dose an ounce, which was equal to half a drachm of chloroform. He again drank after I saw him on the 18th (Monday), and in the evening took one dose of the mixture, or half a drachm of chloroform. He went to bed in his usual state, and lay without sleep. In the course of the night, when suffering under extreme thirst, he stretched out his hand for a bottle of soda-water; in mistake laid hold of the chloroform mixture, emptied it into the tumbler and swallowed it at a draught. It contained four drachms and a half of chloroform. He slept well after taking it, and suffered neither from burning nor pain next day. He was sick in his

stomach on Tuesday and Wednesday, but slept well each night. On Thursday some appearance of jaundice set in. I saw him again on the following Saturday. He was jaundiced and felt slightly sick, but all traces of delirium tremens had disappeared. I could not attribute the supervention of jaundice to the dose of chloroform. It was a complication not unlikely to arise from his habits. We have the main fact, in which at present we are interested, that more than half an ounce of chloroform may be taken into the stomach in a single dose, without causing any poisonous effects. Combination of chloroform with opium will be found, I believe, to form a most useful exhibition in delirium tremens. Its exhibition, on the contrary, by inhalation, in delirium tremens, is of very doubtful utility, and is attended with extreme danger.

Dr. Gordon has, in the February number of this Journal, given some important practical information on trials of its administration by inhalation in fever for the purpose of procuring sleep. The results discourage its use, and as far as my observation has gone, I would also altogether disapprove of its exhibition by inhalation in delirium tremens. The risk to life is very great; two fatal cases have come within my knowledge: in one, death was instantaneous; in the other, the patient sunk into collapse under its administration, never rallied, and died with blue lips and cold surface in less than an hour. Patients suffering under an attack of delirium tremens will not unfrequently suddenly drop dead after a moment of excitement; but still when a patient, even in such a disease, passes into death without rallying from the well-known effects of chloroform inhalation, it appears to me to be sufficient reason for refraining from its use. Even where its inhalation produces sleep, either in fever or delirium tremens, the effect appears to be but transitory. This has been already noticed in Dr. Gordon's observations in respect of fever. In the following case this effect is also illustrated, and although the patient did not die, the escape from collapse was sufficient to prevent re-employment of the remedy.

Mr. K. was seized with delirium tremens, of the asthenic kind, on May 6, 1852. In the course of next day, 7th inst., he had eighty minims of tinctura opii, in camphor mixture, without benefit. On the 8th the quantity of opium was increased; he had sixty minims of tinctura opii in one draught, and afterwards two draughts, with 30 minims in each, without benefit. On the 9th he had a draught every two hours, with thirty minims in each. He took in all eight of these draughts, but still no sleep, and during the night took six draughts more, containing each forty minims of tinctura camphoræ, and thirty minims of tinctura opii. A similar treatment was continued up to the 11th inst. There was still no sleep, the tongue was dry, and the pupils contracted, showing that the poisonous effects of opium were beginning to develope themselves.

In this condition, as opium could be of no further use, and as I was then not sufficiently acquainted with the valuable action of chloroform

given by the mouth, and its freedom from danger, I determined to use chloroform inhalation. It was evident that without sleep the patient must very soon die; and opium had failed to procure it. I used about two drachms of chloroform by inhalation, and with the necessary precautions. The patient became as usual excited, and then suddenly let the head fall back. The lips became blue; the breathing laboured; the pulse became very weak and intermitting; and the pupils dilated. Fresh air, ammonia, and dashing with cold water, kept up respiration and the heart's action; and he passed from this state into quiet sleep; but soon after, awaking at the end of a few hours, the symptoms of delirium tremens returned, putting on, in addition, an appearance of maniacal delirium. This, next day, 12th inst., passed into delirium tremens of the same form as at first, which required for that and next day full opiate treatment. The case recovered; but the risk to life in the case itself at the time of inhalation, the relapse of the symptoms, and the fatal issue in other cases, appear to me to be sufficient reasons for discouraging, very strongly, the use of chloroform by inhalation in delirium tremens; and we can the better afford to dispense with its aid now that we are aware in what considerable doses it will be borne by the stomach; and that while it procures sleep it does not entail risk to life. The external application of chloroform is one of our most useful anodynes, but in the directions for its employment externally, as before noticed of its internal administration, it appears to me that it is not usually ordered of sufficient strength: one part of chloroform to seven or eight of ointment or liniment is not of sufficient strength. I think it will not often disappoint if the liniment prescribed is directed to be made of equal parts of chloroform and olive oil. In this form it will be found to give often great relief in ordinary neuralgic pains, and to remove instantaneously those distressing pains, often described as cramps, really neuralgia, which accompany cholera, and which are so often referred to the sides of the fingers and toes, and described as running along the tendons, very often most acutely felt in parts where there are no muscles to give rise to cramps.—*Dublin Hospital Gazette, Nov.* 15, 1854, *p.* 308.

149.—REMARKS ON THE EXHIBITION OF CHLOROFORM

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[Great difference of opinion exists amongst eminent men as to the safety of this agent. Mr. Syme thinks its exhibition as safe as any other powerful agent, and that its danger depends upon its mode of exhibition.]

In inquiring into the reason of this difference between the experience of chloroform in London and here, we have not far to search for the explanation: it must lie in one of three things—viz., difference

in the chloroform, difference in the patients, or difference in the mode of administration: with regard to the chloroform, I believe that most of that which is used in London is made in Edinburgh, and I know that some of the fatal cases might be shown to have occurred with Edinburgh chloroform.

With respect to the patients, it appears that great care is taken in London to use chloroform only in persons free from chest affections, especially cardiac derangements: here we never ask any questions as to the state of the heart or constitution of the patients. In all cases where chloroform is required for an operation, it is freely given. Now, considering the frequency of cardiac disease, and particularly of fatty heart,—which, in fact, is I believe rarely absent, at any rate in elderly persons,—and considering also the immense number of patients operated on, you cannot doubt that many hundreds with fatty degeneration of the heart have had chloroform administered to them here; we even give chloroform without scruple, where we know disease of the heart exists. Within the last week, a case in point occurred in my practice. A patient, with great dread of pain, and also with a horror of chloroform, had long endured severe pain from his disease, till existence had become a burden, because he could not venture to undergo the necessary operation without chloroform, while his medical adviser considered that to take it would be for him almost certain death, on account of organic disease known to exist in the heart; for which he had consulted, and which it may be remarked had been recognised by Dr. Addison, of Guy's Hospital. At length his medical attendant said to him, that he had suffered so much from his complaint, that even if he died under chloroform this would be better than remaining as he was; while, if it should so happen that he should recover, he would be able to enjoy the rest of his life free from the disease. The patient could not resist the force of this argument, and came to Edinburgh prepared for either alternative. I performed the operation under chloroform; and the first thing that he did on waking was to ask for a cigar.

As another example, I may mention the case of an old gentleman, aged seventy-four, affected with disease of the heart, from whose bladder I removed a large stone, some years ago. Chloroform was administered as he lay in bed; and he was put so fully under its influence, that he was taken from the bed, was operated on, and put back to bed, before he woke from his sleep. He recovered perfectly; but some time afterwards died, and Dr. Begbie, on examining the body, found the disease of the heart, which had been diagnosed during life. We cannot, therefore, attribute the absence of deaths here to our being more discriminating than others in the patients to whom we administer chloroform; the very reverse, in fact, being the case.

I think, then, that we are necessarily led to the conclusion, that the difference of results depends on difference in the mode of administration.

So far as I can ascertain, from what I have heard and read upon the subject, there are important differences between the mode of administration of chloroform here and in London. It appears that here it is given according to principle, there according to rule. There great attention is paid to the number of drachms or minims employed; here we are entirely regardless of the amount used, and are guided only by the symptoms of the patient. The points that we consider of the greatest importance in the administration of chloroform are—first, a free admixture of air with the vapour of the chloroform, to ensure which, a soft porous material, such as a folded towel, or handkerchief, is employed, presenting a pretty large surface, instead of a small piece of lint, or any other apparatus held to the nose. Secondly, if this is attended to, the more rapidly the chloroform is given the better till the effect is produced; and, hence, we do not stint the quantity of chloroform. Then—and this is a most important point—we are guided as to the effect, not by the circulation, but entirely by the respiration; you never see anybody here with his finger on the pulse while chloroform is given. So soon as the breathing becomes stertorous we cease the administration; from what I have learned, it is sometimes pushed further elsewhere, but this we consider in the highest degree dangerous. Attention to the tongue is another point which we find of great consequence. When respiration becomes difficult, or ceases, we open the mouth, seize the tip of the tongue with artery-forceps, and pull it well forward; and there can be little doubt that death would have occurred in some cases if it had not been for the use of this expedient. We also always give the chloroform is the horizontal position, and take care that there is no article of clothing constricting the neck. There are thus considerable differences between our practice and that which prevails more or less elsewhere. We use no apparatus whatever, take the respiration for our guide, attend to the condition of the tongue, and never continue beyond the point when the patient is fully under the influence of the anæsthetic.—*Lancet, Jan.* 20, 1855, *p.* 55.

150.—*Anæsthesia by Cold.* By Thos. Wakley, Esq.—[Mr. Wakley thinks that we may produce a sufficient degree of local insensibility by cold to enable us to do without chloroform in many cases. His words are :]

I have tried the ice in several cases, in both hospital and private practice, and in almost every instance the success was evident, the patient when blindfolded being ignorant of the use of the knife. It affords me great pleasure to confirm the evidence of Dr. Arnott upon this interesting subject, and to draw the serious attention of surgeons, both at home and abroad, to this agent, whereby patients can be freed from the pain which attends many every-day operations we are called upon to treat with the knife, and reserve chloroform for those very severe cases where either the duration of the operation or the depth of structure requires the employment of that potent agency.—*Lancet, Feb.* 3, 1855, *p* 140.

151.—ATMOSPHERICAL OZONE AND THE CHOLERA.

According to Professor Schönbein, the discoverer of cotton-powder, a complete absence of ozone has been remarked in the atmosphere at Berlin during the invasion of the cholera. Dr. Bœckel, of Strasburg, informs us, that in that town also the presence of cholera coincided with the absence of ozone, and that ozone re-appeared as soon as the epidemic decreased.

Dr. Billiard, of Corbigny, is of opinion, that the diminution of ozone in the atmosphere is the first cause of cholera, and that this modification of the air brings forth a change in the animal organisation, in consequence of which, the liquids contained in certain vessels and the substances contained in the digestive tube, are withdrawn from vital action, and only remain subject to the forces by which inert matter is ruled.

Thus the production of a putrid fermentation, the disengagement of gases, and other physical phenomena, by means of which, according to Dr. Billiard, "all the morbid symptoms of cholera, from its period of incubation, to its fatal, or favourable termination, may be explained.

What is ozone? To this question we will endeavour to give a comprehensive answer, as the subject is well worthy of attention, and its assiduous study, added to a daily meteorological observation, promises to be no less interesting to the physiologist and pathologist than it is at present to the chemical philosopher. Ozone is nothing else than oxygen itself, but so different from the body to which, since Priestley's discovery, we are accustomed to give the name, that it has been extremely difficult to find out its real nature.

Oxygen has no smell. Ozone, on the contrary, has a very penetrating odour; resembling, at the same time, that of chlorine mixed with air, and of phosphorus or sulphur in combustion.

It is the odour which manifests itself after repeated electrical discharges. The new permanent qualities acquired by oxygen when it changes into ozone is not confined to its smell alone. Its oxydizing power is much greater than that of common oxygen. The latter combines only very slowly with mercury at an ordinary temperature; the former, on the contrary, unites itself very rapidly with this metal.

Van Marum was the first who found this remarkable body, in 1785. Having at his disposal the great machine of Taylor's Museum, he excited sparks in a tube filled with oxygen. After 5000 sparks the oxygen had acquired a strong smell, "which seemed to us," says this natural philosopher, "to be clearly the smell of electrical matter." From 1785 to 1840, these remarkable experiments were completely lost sight of; but in the latter year, Professor Schönbein, while decomposing water by a galvanic battery, remarked, that the production of the hydrogen gas was accompanied by a peculiar smell, and published a paper on this subject. What was this new body? A simple one,

or a composition of oxygen with hydrogen or azote. The ingenious chemist left the question undecided, but gave the name of ozone to the odorous substance.

Eight years after, Mr. Williamson, a member of the Royal Society of London, pronounced, that according as ozone is produced by a galvanic battery, developed by the electric spark, or brought forth by the action of air or phosphorus, it is a hyper-oxide of hydrogen and azotic acid, or a mixture of the two. Professor Schönbein, had meanwhile continued his researches. In 1850 he expresses an opinion that ozone is a highly oxygenated combination of hydrogen. In 1851, Messrs. Martignac and De la Rive came to the conclusion, after a series of experiments, that ozone is nothing else than oxygen in a peculiar state of chemical activity, impressed upon it by electricity. Berzelius and Professor Faraday also believed in a simple modification of oxygen, in an isomeric or allotropic state of the body. Professor Schönbein, taking up the question for the third time, ranged himself in 1851 to the opinion of M. Martignac and De la Rive. Most chemists, however, still hesitate to admit the modification of oxygen; but the experiments published in 1852 by Mr. E. Fremy and Edmund Becquerel seem to have removed all doubts on the subject.

Thus, ozone is only a peculiar form of oxygen produced by electricity; a change analogous to that which the solar rays bring forth in chlorine by rendering its affinities more powerful, or to the modifications which are excited by heat in sulphur, phosphorus, and carbon.

This fact having once been admitted, the question naturally arose, whether the changes we bring about in our laboratories are not produced spontaneously in the atmosphere; and this was very soon affirmatively answered. Since 1850, Professor Schönbein had ascertained that ozone decomposes iodide of potassium, and concluded that the best re-agent for finding out the presence of ozone is starched paper containing a small quantity of the iodide. Paper thus prepared and exposed to the action of the atmosphere soon revealed the presence of ozone. But it was evident that this singular body could not always be contained in the air in the same proportions; and to study these variations, a scale must necessarily be formed. An ozonometrical measure was easily established by dividing into ten shades the chromatic interval between the absence of ozone, which is white, and the deepest colour which this substance can possibly bring about by decomposing iodide of potassium. By means of this scale, the daily variations of atmospheric ozone may be ascertained in the same manner as those of the temperature and weight of the air are indicated by the thermometer and barometer.

Several men of science, impressed with the importance of this new branch of meteorological observations, have devoted peculiar attention to it. The following table, published by the 'Strasburg Medical Gazette,' gives us the mean of the observations of Dr. Bœckel, of Strasburg, and Mr. Simonin, of Nancy:—

1853.	Simonin. Morning.	Simonin. Evening.	Bœckel. Morning.	Bœckel. Evening.
April	5·93	6·66	5·50	6·33
May	5·71	6·71	5·70	6·60
June	5·55	6.37	4.34	6·25
July	4·44	5·42	4·10	6·50
August	4·39	4·74	4·33	6·18
September	5·23	5·80	3·00	4.83
October	3·94	4·32	3·00	3·50
November	3.50	0·93	1·60	0·50
December	5·16	1·58	4·00	2·00
1854.				
January	—	—	5·35	2.71
February	—	—	7·00	4.50
March	—	—	3·50	4·40

Dr. Bœckel makes the following remarks :—

"In summer, the ozonometrical mean is, according to my observations, constantly more considerable during the day-time than it is at night; the ozonoscopical paper being exposed from morning to evening to two maxima of electricity; while during the night the re-active paper only traverses one maximum period, which is between two and four in the morning.

"In winter, the inverse seems to take place; the electrical affinities being then most likely less energetic, in consequence of the absence of the sun, although experience seems to have proved that a greater quantity of electricity is produced in winter.

"During a fog, the ozonoscope frequently marks zero, as only the rapid formation of vapours, or their precipitation in rain or snow is accompanied by a disengagement of electricity. This took place during the last four months of last year, which were frequently foggy. The ozonometrical mean of these months has constantly been inferior to that of the remainder of the year.

"Thus we have here a body, the existence of which was not even suspected a few years ago, and which never ceases to act upon us, and upon all animated nature. As to the intensity of its action, it cannot be doubted; for who can question that considerable variations in the oxydizing power of respirable gas has a powerful influence upon respiration and consequently upon all the vital functions."

According to Dr. Bœckel, malaria always shows itself with the zero of the ozonoscope, and the same takes place when intermittent fevers are prevalent.

According to Professor Schönbein, a considerable quantity of ozone was observed in the atmosphere at Berlin during an epidemic of influenza, and under a medical constitution predisposing to pulmonic affections; and the contrary took place under the reign of a gastric constitution.

Finally, as we mentioned at the head of the article, the cholera coincided at Strasburg and Berlin with an absence of atmospherical ozone. —(From *La Presse.*)—*Med. Times and Gazette. Jan.* 6, 1855, *p.* 16.

152.—CHOLERA TREATED BY THE ARTIFICIAL PRODUCTION OF PERITONEAL OR CELLULAR DROPSY.

By. Dr. B. W. Richardson, Mortlake.

[Dr. Richardson suggests a novel method to make up for the loss of fluid which seems to be one great cause of the collapse in cholera. Instead of giving fluids by the mouth, or injecting them into the veins, he would inject them into the cellular tissue or the peritoneal cavity.]

As the loss of fluid from the system is the cause of the collapse, as in many cases it cannot be replaced by the natural process of imbibition from the intestinal surface, and as the blood cannot circulate and reaction occur, without such replacement, there is open before us these plain and simple physiological problems—Are there any other means by which fluid can be introduced into the exhausted circulation? and, if so, which of these is the safest, and most calculated to effect the object desired?

We have seen that the mucous membrane of the alimentary canal, while carrying on an extensive excretory process, cannot invariably be made a medium for absorbing fluid to any great extent. Some practitioners have, however, believed that the administration of large quantities of fluid by the mouth, during the intervals of rest from vomiting, has been attended with benefit; and there surely can be no objection to a plan so reasonable, unless the swallowing process should itself excite vomiting, and especially as patients themselves almost invariably crave urgently for drink.

By other practitioners, and by one set of charlatans, the external application of water to the body, has been advocated; and this plan is also reasonable to a certain extent, for it is well known that, at a push, the skin will take on a modified form of absorption. Thus the body has been found in some degree to be nourished by the immersion of its parts in nutritive fluids, in cases where admission of liquids into the stomach is rendered impossible by disease: and in the voyage of Captain Bligh, after the mutiny of the *Bounty*, when the sailors were without drinking water for many days, and were exceedingly exhausted, their pangs of thirst were remarkably assuaged by dipping their clothes into the sea, and afterwards wrapping them round their bodies. I do not know from experiment to what extent the skin will absorb water; but it is quite certain that in cholera, where the loss is so sudden and extensive, and where a supply equal to the loss is required, no amount of immersion could be expected to do marked good, to say nothing of the inconvenience of such an application in cholera cases.

The most important step that has as yet been made on the human subject towards supplying the exhausted system with fluids in cholera, is that of throwing a considerable quantity of fluid directly into the circulation through the veins: and it cannot be denied that, in practice, this plan has been attended with some amount of success. That it should not be generally successful, is, I think, strictly explainable on physiological grounds. In the first place, the plan is objectionable on the mechanical argument, that to throw a large amount of fluid through a vein directly into a heart itself exhausted, is sufficient to paralyse that organ to a serious degree; for I have found, by direct experiment, that a weakened heart often succumbs instantly from over-distending it with blood itself. To meet this difficulty, I have sometimes thought that a long narrow syphon tube, with one end fixed in the vein, and the other immersed in the fluid, would be much better than the syringe; since the suction power of the circulating organ would gradually take in from the reservoir the required element. I throw this out merely as a suggestion, unsupported as yet by experiment.

Another objection to the direct transfusion of water, or of water impregnated with saline or other materials, into the circulation, is, that such fluid is not the proper stimulus for exciting the heart to contraction; and that, even if the heart does contract upon it, and throws it round the pulmonic circuit, it is, again, not a substance upon which the process of respiration can properly act. When a saline substance, injected into a vein, accomplishes anything, it does so, I imagine, by dissolving the half-clotted blood in the cardiac cavities, and by carrying the blood, thus diluted, round the lungs. The mention of this idea indicates how slowly fluids should be introduced into veins, and supports the notion of the syphon tube, alluded to above.

But the last and main obstacle to the success attending injection into a vein, is, that by this process no such competent amount of fluid can be introduced so as to produce a permanent effect: and, in truth, we too often find in practice that, after such injection, the patient only rallies for a brief period, to sink again under a continuance of the discharge from the intestines. I do not know what is the largest amount of fluid that has ever been injected into the vein of a cholera patient; nor yet the largest amount of fluid that has been known to be lost during the disease. Each of these questions ought at once to be ascertained, and might easily be so: the first, by each practitioner keeping a careful record of his treatment of cases; and the second, by weighing patients at the commencement and end of the disease, and by calculating the loss of weight sustained. In hospitals, this last fact could easily be learned, without teasing the patient, by having beds made as weighing machines; taking account of course, of the alterations in weight produced by clothes or other appendages. Speaking from general observation, however, I may say that the largest amount of fluid I have ever heard of as having

been injected into a vein in the course of a case is from seven to ten pounds, and the most I have ever seen injected is from three to four pounds; whilst I have known a cholera patient to pass off by one vomit from two to three pounds, and repeat this many times in the course of three or four hours. I speak on this matter with great caution, and strictly subject to correction; wishing only to convey to the mind the idea that, by the ordinary processes now in use, no sufficient quantity of fluid can be injected by a vein to make up a tithe of the serum lost, which is the point to be desired. I make no mention of any of the risks that may arise from injection into a vein, such as introduction of air or phlebitis; because these, though important in themselves, are of secondary consideration in extreme cases of cholera, when compared with the main object of diluting the blood and re-establishing the circulation.

In contemplating the subject of transfusing watery matter into the system after the modes I have described, the question occurred to me, whether we ought to confine ourselves to these modes; or whether any other means existed of introducing fluid matters into animal bodies in very large quantities, and in a manner which should secure their absorption. This thought led me to look back upon the pathological characters and treatment of those diseases in which serum is largely thrown out into serous cavities or cellular tissue. I recollected that in cases of this class (dropsical cases,) not only were several gallons of fluid thrown out of the circulation into the system without immediate danger, but that patients thus situated could tolerate a degree of purgation which would absolutely destroy healthy individuals. This immunity must arise solely from the fact that the effused serum lies as a reservoir, from which the circulation is fed while the purging is going on. The idea furnished an important suggestion; and I consequently commenced a series of experiments on animals, which, as they are as yet in an imperfect state, I shall only describe in general terms, reserving the particulars for my next report.

The experiments up to this time have mainly consisted in exhausting dogs and cats by starvation and violent purgation with large doses of elaterium, and afterwards injecting either their peritoneal cavities or their cellular tissue with large quantities of distilled water, at the blood temperature. The results, in general terms, are as follows:—

I find that either into the peritoneal cavity, or into the cellular tissue, a quantity of water, varying from a tenth to the fifth part of the weight of the animal may be injected with little risk. The effect of this is to induce a sleepy condition, which lasts from twenty to thirty hours, long before which time all trace of the injection is lost. If more than this is thrown in, the sleep, or rather torpor (for the animal only remains quiet, and rouses when spoken to,) ends in death. This was the case in one dog weighing ten pounds, into whose cellular tissue I injected three pounds of water. In animals

that have been much exhausted, more fluid may be injected, and the absorption is greatly quickened. In animals which have died from the experiment, and have been examined immediately after death, I have never found any trace of the injected liquid; but the blood has always been exceedingly fluid, and has coagulated slowly, if at all. I should notice also, that in two cases, in which a cat and a dog were operated on while under the influence of an anæsthetic agent, they died; the one in four, and the other in forty-eight hours, without ever rallying fairly from the effects of the narcotic. I should mention, too, that I have met with one or two casualties in the course of these experiments, which have arisen purely from their novelty, and cannot be considered as bearing on the merits of the system. Thus, in two young cats of the same age, I injected a sixth part of their respective weights of water into the cellular tissue of one, and into the peritoneum of the other; the result was, that the one whose peritoneum was injected, after remaining apparently well until three or four hours previous to death, died about twenty-four hours after the operation. On opening the body, all trace of fluid was absent; but I found that, from having made too large an incision into the peritoneum, and from the animal having reclined on the belly, about two inches of intestines had slipped into the opening, and had become strangulated, and deeply congested. In another instance, where I inserted pins into two openings, erysipelatous inflammation came on in the surrounding skin; but not in another part of the body, where the wound was left untouched. In a third casualty which occurred to me, I learnt a very useful lesson. Into the peritoneum of a dog three pounds of water were thrown, and one pound into the cellular tissue of the thigh. The animal seemed pretty well for a short time after the operation, and the fluid was absorbed in the course of three or four hours, although it constituted a fifth part of the weight of the whole body. In a little time, the creature gradually became exhausted, and died in eleven hours. On opening the body, I found that all the fluid injected had disappeared; but in the peritoneum there lay about a pound of semi-coagulated blood, which had escaped from an artery that had been wounded by the trocar. The blood in the veins and heart of this dog was like water itself, and showed no tendency to coagulate. The heart was flaccid and collapsed. In this case, the rapid absorption of the injected fluid arose doubtless from the hemorrhage, and from the too rapid and copious flow of fluid into the exhausted circulation. For, while the blood lost its solid constituents on the one hand, it gained too large an amount of water on the other. This animal had been reduced somewhat by purgation. Previous to the operation, he weighed twenty-six pounds; immediately after it, thirty pounds; and, at his death, a little more than twenty-four pounds. He must consequently have made use of all the injected fluid, and have lost somewhat more than one pound additionally, during the interval between the operation and death.

Setting aside, therefore, these casualties, I infer from my experiments as they now stand, that into the peritoneum or the cellular tissue of a patient in a proper state of collapse from cholera, water to the extent of at least a tenth, or even a fifth, part of the whole weight of the body might be injected with safety; and that the absorption would be almost immediate.

The performance of the operation for producing this artificial dropsy is exceedingly simple. The instruments required are simply a scalpel, lancet, or trocar, a common catheter, and an ordinary enema syringe. If the cellular tissue is to be injected, a deep puncture is made with a lancet in some fleshy part, the abdomen is the best, and the blunt point of the catheter is then introduced, and wedged sufficiently far into the cellular substance to allow the skin around it to be firmly grasped, so as to keep it steady in its place. The other end of the catheter being connected with the enema pump, an assistant very gradually throws in the fluid, and, as the tissue distends, gentle compression may be made with the hand so as to force on the fluid under the skin. Into the leg of a dog of middle size, a pound and a half of water may thus be injected in a quarter of an hour; and the experiment may then be recommenced at some other point if desired.

For puncturing the peritoneum, I am now having made a special trocar, which I will describe at a future time; but the scalpel or lancet, if used with care, answers exceedingly well. A careful plunge into the cavity is made in the linea alba; and the blunt point of a catheter is introduced, and grasped as before mentioned. Fluid may in this case be thrown in with much greater rapidity for a considerable time; but I have observed that, as the body distends, the action of the diaphragm becomes somewhat interfered with, upon which the operation should cease for the time. I have injected three pounds of water into the peritoneum of a middle sized dog before producing this effect. The wound must be carefully closed with stitches; but a little of the water will always escape, and become diffused in the cellular tissue of the abdomen.

The steps of the operation in the human subject would not vary materially from those I have already described. The opening into the peritoneum should be made with a small scalpel through the linea alba on the left side, about two inches below the umbilicus: the blunt end of the catheter should then be introduced and steadily grasped, the fluid being meanwhile made to pass slowly through it, so as to prevent the injection of air. The freeness with which the fluid flows through the tube will show whether or not the peritoneum has been freely entered. For injecting the cellular tissue, some point of the body should be selected where the parts are loose, such as the abdominal parietes, or the back; in these parts the cellular substance is extensive, and would offer but little resistance to the passage of fluid.

I am not at this moment able to say which of the two kinds of operation is best, but I am strongly inclined in favour of injecting the

peritoneum; for, although the absorption of fluid from the cellular structure seems most rapid, the difficulty of introducing water in full quantities into the peritoneal cavity is much less, and the pain attendant is considerably milder. In either case, too much care could not be taken to throw the fluid in slowly, to have it at a temperature not less than 92° Fahrenheit, and, if the peritoneum is the part injected, to be mindful not to distend the cavity to the extent of interfering with the action of the diaphragm.

As yet I have had no favourable opportunity of trying the suggestion I have here thrown out in cholera; but that it might be put into application in desperate cases and might prove advantageous, I have no doubt whatever. The advantages of it would be:—

First. That an abundant store of fluid would be supplied, which the exhausted circulating system would rapidly and effectually take up.

Second. That as this imbibition would take place through the capillary system, the fluid would enter the veins freely charged with the solid constituents of the blood, and would pass to the lungs in a condition suitable for respiration.

Third. That the process would in no way interfere with the administration by the mouth of astringent solutions or such other medicines or liquids as the practitioner might consider indicated.

But, although I am thus sanguine on this subject, I wish it to be understood that the first trial of this process should commence in a case where the patient is in the last stage of collapse, where other remedies have failed, and where death is imminent. This, it is true, would scarcely be putting the new remedial measure to a fair test; but my ideas of the value of life are so great, that I would not, even in so important a disease as cholera, recommend the employment of a new remedy wantonly, or without that due care for the welfare of the sick man which every honest practitioner of medicine intuitively feels.

The subject once started, branches off in various ways, and opens many points for investigation. If simple water can be taken into the system through the cellular tissue or peritoneal membrane, it remains to be ascertained whether nutritive or medicinal substances, which could not obtain admission by any other way, might thus be introduced. It is interesting and important to know, that the peritoneum has been found capable of absorbing some medicinal agents. I am informed by Dr. Cormack, that many years ago, he, and the late distinguished Professor of Anatomy at St. Andrew's, Dr. John Reid, threw into the peritoneal cavities of dogs various medicinal substances; and that the proofs of the absorption of these substances were speedily manifested. Dr. Christison, also, in his 'Treatise on Poisons,' p. 29, thus remarks:—"The serous membranes possess an activity of absorption which hardly any other unbroken texture can equal. Accordingly, many poisons act much more rapidly through the peritoneum

than through the stomach. When oxalic acid is introduced under the same collateral circumstances into the stomach of one dog and the peritoneum of another, the dose may be so apportioned that the same quantity, which does not prove fatal to the former, kills the latter in fourteen minutes."

It may be urged that the fact of injecting either the peritoneum or cellular membrane could not be performed without some risk of exciting inflammatory action. And that this would often be the case in *healthy individuals* I have no doubt. But in the collapse of cholera, when all the great processes of the economy upon which inflammation depends are in a state of abeyance, such a result is in no way probable; and if it were, it ought under the circumstances to be regarded as a matter of secondary consideration.

I should scarcely omit to refer, in conclusion, to the fact (though it is one pretty generally understood) that impostors in feigning diseases, have been known to inflate their own cellular tissue or that of their children with air, without any harm following; and the authors of the article, "Feigned Diseases," in the 'Cyclopædia of Medicine,' make special reference to the statement, that some French conscripts were known to inject water into their own peritoneal cavities, to produce a factitious ascites; while numerous instances have lately been recorded, in which, after the operation of tapping, such irritating substances as iodine and brandy have been thrown into the peritoneum without leading to fatal results.

Dr. Goolden, physician to St. Thomas's Hospital, expressed the high opinion which he entertained of Dr. Richardson's paper. He thought, however, that it was a great mistake to attribute collapse to the serous vomiting and purging. He had seen many cases of fatal collapse in cholera, in which there had been neither one nor the other of these occurrences. How, then, could this state of collapse be obviated or relieved simply by the introduction of fluid into the system? In support of his views, Dr. Goolden referred to the nearly uniform fatality of the cases treated by injection into the veins. He however advocated the introduction of large supplies of fluid into the stomach, as he found that in this way sickness was allayed. He would give ice, or water at an ordinary temperature. In 1832, when in a remote part of the country with a party of friends, he saw a case of cholera which had made a strong impression on his mind. The woman of the inn where he was with his friends was seized with cholera, and rapidly became cold and collapsed. No medicine chest and no drugs of any kind could be procured, and at last a messenger was sent off seven miles in search of a doctor, but no doctor and no medicine was obtained. The woman called urgently for water. A pail of water was brought to her bedside, and he (Dr. Goolden) threw into it, he hardly knew why, the contents of a saltcellar. Cupful after cupful was taken, till the woman had finished all that was in the pail. It was then

refilled, and this second supply she also drank. Altogether she swallowed about two gallons of this solution of salt. After the first few draughts the sickness abated; and her recovery was speedy and perfect, though she took no medicines of any kind. Dr. Goolden enlarged upon the mortality which arose from too active drugging, both medical and domestic. During the present epidemic, thirty-six cases of collapsed cholera had been admitted into St. Thomas's Hospital; and of these twelve only had died. But what was most deserving of notice was this, that in twenty-one of those cases in which no drugs had been given there were only six fatal cases. The following plan of treatment is now adopted at St. Thomas's. An emetic of ipecacuanha is first administered: then from half a drachm to a drachm of sulphuric acid in peppermint water is given every half hour or hour, until the vomiting and purging cease. When the vomiting is obstinate, a few drops of creasote are given. Sinapisms, warm fomentations, and thorough covering with blankets, were also means always employed. The patients were allowed to drink water *ab libitum*. In the previous epidemic "secondary fever" had carried off two-thirds of the cases: in the present epidemic not one case had been lost in St. Thomas's Hospital from this cause. This was probably owing to opium and other dangerous medicines, so freely employed on the former occasion, not having been employed on the present.

Mr. Carpenter, of Croydon, called attention to the manner in which the red corpuscles of the blood were affected in cholera. These corpuscles performed the function of carrying oxygen from the lungs through the arterial system, and of carrying back carbonic acid through the venous system to the lungs. When, as in cholera, a large quantity of fluid is lost from the blood, the corpuscles are arrested in their circulation; they contract, and in the stage of collapse they become altered in their chemical characters. Injection into the veins had been unsuccessful, because the red corpuscles were over-distended by the sudden introduction of water, and burst. It was a desideratum to find a means of introducing fluid, which should not be liable to this objection; and this was in some degree afforded by the process described by Dr. Richardson. The difficulty presented was the want of albumen, and of correspondence in the specific gravity of the fluid injected with that of the blood. In the treatment, he objected to terebinthinates and ethers, as liable to produce secondary fever; and he would trust rather to the administration of cold water in abundance, and mild saline solutions.

Dr. Cormack considered the paper of Dr. Richardson as strictly philosophical and extremely able. The new proposal which it conveyed to the profession was a step in the right direction, and was in accordance with that sounder practice in cholera which was now becoming general. He (Dr. Cormack) could not concur with Dr. Goolden, in thinking that the collapse of cholera sometimes occurred when there had been no pouring forth of serous discharge from the

intestinal mucous membrane. He had never seen such a case: he had never been able to hear of any such case well authenticated. Though the discharge did not appear outwardly, by vomiting or purging, it might on dissection be found absolutely to fill the intestimes. This was recorded by various observers. Again, the vomiting and purging was generally far advanced or almost finished before cases were received into hospitals: and the fact of no discharge being reported was not satisfactory evidence that none had occurred. On what does the shrunken, senile, mummified aspect of the collapsed patient depend? It can only depend upon the loss of fluids from the body! If it be true, then, that the loss of fluid leads to collapse, it follows that the prevention of that loss must form an essential part of our treatment. It likewise is evident, that if from the severity of the case or the neglect of early treatment, the case had advanced into collapse, a rapid supply of fluid is obviously essential. A little seasonable aid of this kind may help the patient over his difficulty. We do not throw in the fluid to *cure* the cholera! Our object is to enable the patient to outlive the paroxysm, which we know must soon terminate either fatally or favourably. The cholera paroxysm is similar in kind to, and not generally more prolonged that that of an ague. The fluid artificially introduced, it appears, need not be exactly similar to that which is lost; but the nature of the fluid to be used with most advantage we cannot yet exactly determine. Upon the whole he (Dr. Cormack) would with Mr. Carpenter prefer simple water, if the peritoneum was to be injected; and an abundance of water slightly salted with common salt, or acidulated with sulphuric acids, if the patient were to drink it. The chief point, however, seemed to be to give plenty of fluid. Some medicines were useful, but fluids could never be dispensed with. Ice and cold beef-tea (not strong) he generally ordered *ad libitum*. Dr. Cormack concluded by referring to the case detailed by Dr. Goolden as being full of instruction. Cases of such severity are generally so much interfered with by treatment that they rarely convey much information. The recoveries which take place under opposite methods of treatment, and such cases as that of Dr. Goolden, clearly establish the paroxysmal character of cholera attacks, and point out that such attacks may spontaneously terminate in recovery, just as do attacks of simple ague.—*Association Med. Journal*, *Aug.* 25, 1854, *pp.* 765, 768.

153.—CHOLERA TREATED SUCCESSFULLY BY SALINE INJECTIONS.

By Henry Ancell, Esq.

[We give the following case to show the effects of the saline treatment in recovering from an apparently hopeless state of collapse. The woman was 44 years old, the upper housemaid in a private family. Mr. Ancell says]

August 16th, I first saw her at half-past 11, a.m., six hours after the attack, in a state of complete collapse; there had been violent vomiting, with perfectly colourless fluid dejection, containing rice-like grains and white flocculi; violent cramps, and total suppression of urine; she was pulseless, with a feeble, scarcely audible, fluttering heart, lividity of the countenance, coldness and blueness of the extremities, coldness of the tongue and breath, and clammy perspiration. Frictions of the surface were ordered, and sinapisms to the arms, legs, and epigastrium. Brandy, ether, and ice, and several other remedies were given with a view to restore the circulation, all being rejected. Up to half-past two o'clock there had not been the slightest sign of reaction; the lividity and coldness of all the extremities increased, the respiration was hurried, the countenance more collapsed, and dissolution was manifestly impending, although her mental faculties were entire, her voice tolerably good, and she could reply to questions. A case in the neighbourhood, in which the collapse was not so complete, terminated fatally on the previous day, four hours after I saw it; and from my experience of the epidemic in the year 1832, my opinion was that under all ordinary modes of treatment, or if left to itself, this case would probably terminate in an hour or two, and could not be much longer protracted.

Under these circumstances, with the co-operation of my friend Mr. Lane, I proposed the injection of a saline fluid into the blood. The patient at first withdrew her arm and feebly objected, having always had a prejudice against bleeding and operations of every kind; but was soon overruled. A solution of about one drachm of chloride of sodium in a pint of water, at about blood heat, was accordingly injected into the basilic vein. During the injection the pulse returned to the wrist, but was extremely feeble, and shortly afterwards she expressed herself less faint; this was followed by a distinct rigor, which lasted perhaps ten minutes; and this by an improvement of the countenance, a slight degree of warmth to the extremities, and a complaint of pain in the back and head. After the rigor she had very little cramp; but the vomiting and diarrhœa continued with unabated violence.

At six o'clock, about three hours after the injection, the collapse had returned. Diluted sulphuric acid and laudanum, and every other remedy given to avert the vomiting and purging, had proved unavailing, and she was again pulseless, cold, and livid. The tube had been left secure in the basilic vein, and in a feeble and gasping tone, she begged that she might have some more of the fluid. I now injected about two pints more of the solution of chloride of sodium somewhat stronger. The pulse returned, and during the operation she twice or thrice complained that it made her back ache; there was no rigor, but the reaction appeared from the first to be more complete than before; nevertheless the purging continued unabated; if the stomach was allowed to remain at rest by giving nothing, the vomiting returned at intervals, and everything taken into it was immediately rejected.

The pulse continued at about 110 and tolerably strong, until near 11 o'clock; I then watched its rapid failure in power until 12 o'clock, when it was again nearly imperceptible. The lividity of the countenance and gasping respiration returned; the action of the heart was nearly imperceptible; she was excessively restless, and intimated that the fluid had run off by her bowels, and that she wished for more. It appeared to me, that the effect had shown either that a simple solution of chloride of sodium was not adequate to the restoration of the normal fluidity of the blood and the circulation, or that the quantity injected was insufficient; and I therefore resolved to employ a stronger solution in larger quantity, and to fall back upon a mixture of salts employed by several practitioners in 1832. Five pints of a solution composed of 64 grains of chloride of sodium, 64 grains of carbonate of soda, and 32 grains of chlorate of potass to each pint of water, (16 ounces), were now injected; half a grain of iodine being added to the five pints. The pulse returned, as in the former operation, but was much stronger, and the remarkable and satisfactory result followed, that instead of remaining stationary and then failing, it continued to improve from the period of the injection: so that at four o'clock in the morning, the action of the heart, the pulse at the wrist, the temperature, and the general aspect of the patient so much improved, that I removed the tube from the vein. The vomiting and purging continued, although somewhat abated.

Aug. 17th. Three o'clock, p.m., twenty-four hours after the first injection of the veins, about thirty-six hours from the attack, after the continuance of a state of apparently hopeless collapse for at least four hours, and the subsequent injection of from seven to eight pints of a saline fluid into the circulating current with a view of restoring the circulation, the patient was in the following state:—the action of the heart perfectly regular and uniform; the pulse 96, tolerably strong and regular; the lungs crepitant throughout, and the respiration good; the temperature of the surface moderate; the lividity of the countenance replaced by a deep sallowness; pain in the epigastric and præcordial regions; great thirst; the purging and vomiting unabated, and the respiration slightly hurried immediately before and after the vomiting; no clear evidence of any return of the renal secretion, and the choleraic character of the dejection unchanged.

Sinapisms were reapplied to the epigastrium and to the extremities, and during the day various remedies were resorted to for the purpose of arresting the vomiting, none of which appeared to do any good except ice, which was most grateful, and was taken in considerable quantity. In the evening I prescribed fifteen grains of trisnitrate of bismuth in a little barley-water, followed by some ice, to be given every hour to eight doses should the vomiting continue, and large nourishing enemata, consisting of warm milk and beef-tea, to be repeated several times during the night.

Aug. 18th. The patient had, upon the whole, a much better night.

The reaction was completely established, the pulse 110, and rather full. The vomiting nearly ceased after taking five doses of the trisnitrate of bismuth. She had retained some ale, and also some port wine, on the stomach. The purging continued, but was less frequent, and two of the enemata were retained for about an hour. The alvine evacuations had changed to a bright yellow colour, were much less liquid, but slimy with green streaks. The sounds of the heart were quite normal; the respiration was a little hurried previous to straining to vomit, which occurred occasionally, when a small quantity of yellowish-green ropy mucus was ejected. The tongue, which had hitherto been perfectly clean, was now white; there had been a little sleep; her intellects were perfectly clear; but she complained of great weakness, and was restless.

Aug. 19th. She had a very bad night, with diarrhœa and vomiting, although she retained some warm brandy and water in the stomach; violent pain over the whole abdomen; great flatulent distension, particularly around the umbilicus; pulse rapid; tongue brown and dry; hiccup; cold perspiration; complexion and general surface yellow; extreme restlessness; deep moaning; occasional slight wandering. A large hot linseed poultice was applied over the abdomen, and a turpentine enema administered; but she became listless for an hour or two, and died at two o'clock; three days and eight hours from the attack, and nearly three days from the first injection of saline fluid into the basilic vein.

Autopsy, seventeen hours after death.—The body was plump and in good condition; its surface remarkably free from lividity, except in the decumbent parts, where the sugillation was not more than usual at this period after death; and the *post mortem* rigidity was well marked. The *fat* was nearly an inch and a half thick under the abdominal integuments. *Muscles* of a remarkably good colour. The *heart* was rather large and flabby; a small quantity of limpid fluid was found in the pericardium; the left auricle and ventricle were collapsed and empty; the right auricle and ventricle were loaded with a gelatinous coagulum, a portion of which was black and tar-like, a portion parti-coloured, and another, probably that which at the period of death was in the highest position in relation to the other portions, was colourless. The coagulum extended a considerable distance into the pulmonary artery, the greater part being colourless; it also surrounded and adhered to the valves and chordæ tendineæ, but was easily peeled off. The *lungs* were collapsed, crepitant, without the slightest sign of disease, and with rather less than the usual hypostatic congestion. The *stomach* contained about five ounces of a yellowish green, thick, ropy fluid; its mucous coat was softened, thickened, easily scraped off, and covered with a thick ropy secretion; there were large ecchymosed spots of a dirty brown colour, in the subjacent textures, particularly around the pyloric extremity; and the large vessels in the sub-peritoneal surface were congested with dark

blood, in some of which the blood was coagulated. The *small intestines* contained a large quantity of greenish-yellow ropy fluid; their internal coat was thickly lined with a tenacious yellow ropy fluid, with difficulty washed off; the mucous membrane thickened, softened, and easily peeled of; the subjacent tissue highly vascular, from the pylorus to the ilio-cæcal valve, more especially throughout the duodenum, where it nearly resembled a piece of scarlet cloth. Numerous enlarged glandulæ solitariæ in the course of the ileum. The *large intestines*, particularly the colon, were greatly inflated, and exhibited morbid appearances, in a minor degree, but similar to those observed in the small intestines; they were loaded with a yellow semifluid substance. The liver was somewhat friable and pale, and the blood pressed from several of the hepatic veins permeating it resembled coloured water; under the microscope, it presented a very decided morbid appearance, having far less colour than blood-proper, and the red corpuscles being irregular in shape, unequal in size, and apparently imperfect, dissolved, or otherwise damaged. The *gall-bladder* was distended with dark green bile, becoming yellow on dilution, and the ductus choledochus filled by concreted bile. *Spleen* healthy. *Kidneys* healthy, except that in one a very small quantity of an apparently purulent fluid could be pressed from the tubuli uriniferi. The *bladder* was completely collapsed. A tumour twice the size of an orange replaced one of the ovaries. The *blood* in the large vessels generally was tar-like and gelatinous.

Remarks. I have related the circumstances of this case precisely as they occurred, believing that considerations of the deepest interest at the present moment are involved in them. In the first place, I have to explain the causes of some of the imperfections in the proceedings narrated. Called upon suddenly, in a case of the utmost severity, where no time was to be lost, occurring in a private house, I had to obtain as quickly as possible the necessary co-operation, the solution of salts, and, in the absence of any appropriate injecting apparatus, the best instrument I could contrive. My fear was that the patient would die before the operation could be performed. The salts were dissolved in distilled water, and the solution filtered; but it had to be warmed by dilution with hot water from the cistern in the house. I was unprepared with a thermometer at the time of the first two injections, and the temperature was judged of by my own sensation. Mr. Talbot, of St. Mary's Hospital, and myself made the *post mortem* examination, without any other assistance; and, from circumstances relating to the family, we were limited to time. It was accordingly done imperfectly, at least in my view of the points which ought to be made out in such a case. If my own opportunities of investigating the remedial powers of the direct admixture of substances with the blood in this fearful malady were greater than they are, I should postpone the publication of this case until I had made a series of experiments, not only more carefully and exactly, but on a less empirical—a more systematic principle. I

have adopted the other course, because I believe the facts are eminently suggestive, and calculated to guide others, possessed of better opportunities, in the conduct of this important inquiry.

Here is a case in which the patient was perfectly restored from the collapse stage of cholera in its most intense degree. Although eight pints of a saline solution, by no means of the purest character, either as respects the salts or the menstruum, had been mixed with the blood in the vessels, the patient lived three days, and no disturbance was produced by it either in the lungs, the brain, or as far as evidence goes, in any organ or structure. The *post mortem* morbid appearances were strictly those incidental to cholera; the lungs were specimens of healthy lungs; and the intellectual faculties were perfect until the patient was moribund from the fully developed disease. Then, what an interesting tale does this case unfold of the sequel to the collapse. Whoever has had an intermittent fever will well remember the blueness of the nails, the aching of the fingers' ends, the state of the features and of the surface of the body, the miniature collapse that constantly preceded the rigor; and here we have the intense collapse of cholera apparently overcome by an antidote, and followed immediately by a rigor. This rigor is said to "usher in" various diseases, but it is almost always a secondary phenomenon, the commencement of reaction. The reaction established, we find, in the case before us, all the symptoms of the most intense gastro-enteritis, the more prominent of which had, indeed, existed from the commencement, and from their intensity had aggravated that collapse which is the primary effect of many poisons, and particularly animal poisons introduced into the blood. Even the mineral poisons, as, for instance, arsenic, in deadly doses, however introduced into the blood, produce effects very closely resembling the symptoms of cholera. We see in this particular case, also, the extraordinary intensity of the gastro-enteritis, exhausting and destroying the patient, but not until pretty clear indications had been given as to what would have been the result had it been less intense, and had life been still further prolonged; the clean tongue of the stage of rapid and sudden collapse was succeeded by a white fur, as soon as the vomiting abated; and this was very rapidly followed by the dry brown tongue of typhoid fever. To comprehend these remarkable circumstances, we must avail ourselves of the little that is known of the effects of poisons introduced into the blood. In Gaspard's experiments, recent pus injected into the blood of dogs produced abundant urine, reiterated vomiting, fœtid stools, tenesmus and other symptoms, and death in a few hours; the intestines were thickened, and the rectum and colon especially inflamed. Among the symptoms resulting from the injection of fœtid pus were great prostration, vomiting a blackish liquid, and extremely fœtid dejections; or, when the quantity was larger, frequent whitish very fœtid stools, ardent thirst, great prostration, hiccup, dyspnœa, violent palpitation, &c.; and after death the blood was very coagulable with

petechiæ, &c. Animal putrilage produced bilious, gelatinous, and then bloody vomiting, gelatinous and bloody dejections, or the most violent dysentery, or black vomiting; and in small quantities, all the symptoms of malignant fevers; and, after death, the stomach, duodenum, or rectum, were inflamed. Vegetable putrilage produced the symptoms of dysentery, malaria, and low fever, and the mucous membrane of the intestines, especially of the duodenum, rectum, and some of the small intestines, were of a violet-red colour, and inflamed.

The more minute details of the experiments indicate that each distinct poison produces a specific effect in the blood and tissues, and is attended with its peculiar symptoms, among which prostration, partial or general gastro-enteritis, and the symptoms of typhoid or malignant fever, are the most common. There is sufficient analogy in the symptoms of cholera to lead to the inference that it is a poison in the blood, producing more intense sedative effects in the blood itself, and a more violent determination to the gastro-enteric surface, than any known poison; and sufficient distinction, to lead to the inference that it is a poison *sui generis*, and totally distinct from all other poisons, whether known only by their effects, or otherwise. Our present knowledge of the physiology and pathology of the blood renders both the analogy and the distinction quite intelligible. Although in some cases of cholera the recovery from the state of collapse may be quickly followed by health, inasmuch as the poison may have expanded its virulence, and the gastro-enteric mischief may be slight, yet in these cases of fearful intensity such a result is scarcely to be hoped for; the recovery from collapse is only the recovery from the early stage of the disease, and we may expect it to be followed by gastro-enteritis; or by pneumonia, or some other result of local congestion or disorganization; or by typhoid or malignant fever, complicated with some of these local effects; and a remedy for the collapse ought not to be regarded as a remedy for the disease. The purging and vomiting, at the outset, are no doubt the effect of the disorganisation of the blood; and the intestinal canal is the outlet both of the poison and of the devitalised portions of the blood; but I conceive that it would be an error in pathology to regard these symptoms, even from a very early period, in this point of view simply; they depend also upon the irritation or inflammation of the coats of the alimentary canal, set up during the elimination of the poison; hence the dejections, even from the commencement, contain a large portion of epithelium scales, and the disorganisation bears, to some extent, a proportion to the intensity of the poisoning. In Mason's case, I conceive the quantity of the poison originally received into the blood must have been very large; but for the remedy employed, it would have destroyed life in less than twelve hours by its direct effect on the blood; failing this, it produced this result by the intensity of the gastro-enteritis, and probably, in part, by the poison not having

been completely eliminated. Had the quantity of the poison originally received been less, the probability is, *cæteris paribus*, the gastro-enteritis would have been less, but on recovery from the collapse we should then have had a fully developed typhoid fever to contend with. On the other hand, had the quantity of the poison been less, and had it also been completely eliminated, during the progress of the collapse, (I believe, a frequent result), there would have been a less complicated gastro-enteritis to deal with. As recovery often takes place very rapidly, even after collapse in a slight degree, it is probable that the gastro-enteritis set up during the elimination of the poison, or any effect produced during the collapse, is, in such cases, comparatively trifling. I have since witnessed the autopsy of a fine young woman rapidly destroyed in the stage of collapse, wherein the intestinal affection, although well marked and general, was still slight in degree; and, no other organ being materially affected, the suggestion presented itself, that could she have been carried through the collapse, her recovery would have been rapid. In the slighter cases, in which the quantity of the poison received into the blood is very small, producing diarrhœa only, the poison is probably very quickly eliminated, and the blood not being so extensively disorganised, nor so quickly inspissated, if the elimination by the bowels be checked, as it usually is, by the remedies we employ, the remaining portion of the poison is quickly destroyed or discharged by other outlets, and the collapse, with all the other consequences, are averted; and even where the quantity of the poison is larger, the greatest advantage results from arresting the purging in the early stage before the blood becomes inspissated, inasmuch as the rapid loss of the fluid parts of the blood is doubtless concerned in preventing the elimination of the poison by other organs.

We know little of the laws which regulate the action of poisons received into the blood, either upon the blood itself, or upon the whole animal economy through the blood. Nevertheless, it appears that each has its specific effect, while there are effects common to nearly all. The choleraic poison appears to act in the blood as a sedative. There is some reason to think that it arrests the ascensive metamorphoses of the albuminose, so that all the albuminate of soda of the liquor sanguinis is not normally developed, since the latter will not permeate living membrame, which the former does. That it acts by an arrest of development in the materials of the liquor sanguinis, rather than upon the blood proper, appears plausible also from this—the retention of the urea in the blood is the usual result of the kidneys failing to separate this substance; and not only does this take place in cholera, but according to some of the best analyses, the urea accumulates, indicating the continuance of those metamorphoses in the blood by which nitrogenous compounds are converted into urea. Again, the red corpuscles appear to remain, up to the latest moment of life, without any serious deterioration; else a saline fluid could not with its other effects so instantaneously restore the functions of the blood.

The corollary of these remarks is, that a saline fluid, mixed with the blood by injection into the veins, has the certain effect of *prolonging life* in the worst cases of the collapse stage of cholera; even where, from its intensity, the disease must necessarily prove fatal. Since our mission is, under all circumstances, to prolong life if possible, it is certainly justifiable, in otherwise hopeless cases, to perform this operation; and it even becomes questionable whether we ought to allow any such case to succumb without resorting to it. It is also a fact that a few individuals have recovered from an otherwise hopeless state of the disease after this operation. Were it resorted to more generally in cases hopeless of recovery, on the principle of prolonging life, our efforts would very soon be directed to learn the best mode of operating, as respects the quantity of fluid to be introduced, the nature of the fluid, the times at which it might be repeated, &c., with a view to obtain the utmost prolongation of life possible: and I am sanguine enough to believe that the accuracy of observation which this would induce must lead to important results in relation to the cure of the disease. The primary object, as respects this operation, is to discover the proper remedy for the collapse, taking care that the effects of such remedy shall not be incompatible with recovery from the disease, and the restoration of the patient to health. Viewing a patient in the most extreme state of collapse from cholera, it should always be borne in mind that in such a patient, until life is extinct, there does not *necessarily* exist any disorganization totally inconsistent with recovery. Finally, I have seen a man pulseless, speechless, cold, and livid, rise up in bed and converse, after the injection of a saline fluid into the blood, and remain for several hours capable of declaring his last wishes to those around him, or of taking his farewell of the world, of forgiving or being forgiven, or, it might be, of making his peace with his God.—*Association Journal, Aug.* 25, 1854, *p.* 760.

154.—*On Cholera.* By Dr. Ottinger.—Dr. Ottinger says: "Epidemic cholera is to be considered as a disease, based on an alteration of the vegetative nervous system, (a morbid affection of the sympathetic nerve,) which rapidly terminates into paresis, or even paralysis of the vaso-motor nerves. In consequence of a morbidly increased exosmotic process, the serum of the blood exudes through the intestinal canal, occasioning desquamation of the intestinal epithelium. A remedy is, therefore, indicated, which exerts a most stimulating effect on the vaso-motoric nervous system." Valerianate of ammonia has proved itself, according to his experience, to fulfil the above indication, with an appropriate external treatment. He gave no other internal remedy to his cholera patients in the present epidemic. If the treatment was commenced before the appearance of collapse,—conditions of constitution and age not being previously unfavourable,—the result was most satisfactory in the majority of cases. Even

after the pulse had been imperceptible for hours, after the face and extremities had exhibited icy coldness, &c., &c., re-action was established, and recovery ensued, without any subsequent cholera typhoid.

He administered every quarter or half-hour a tablespoonful of the following mixture:—

℞. Valerianat. ammon. ℈j.; aq. destill. ℥iij.; syrup. simp. ℥ ss.

When re-action set in, the remedy was given in longer intervals. Externally he caused frictions to be made every hour, alternately with ice or Burgundy vinegar. These were invariably followed by constant rubbing with hot flannels. If, notwithstanding these means, the cutaneous warmth did not return, he ordered a hot-bath (from 23° to 33° Reaum.), with an addition of an ounce or an ounce and a-half of cautic potash.

As a dietetic beverage, he gave the patients ice-water, Seltzer-water, or claret, besides ice pills. The undoubted success of this treatment in six cases (four of which were decided cases of asphyxiated cholera), encouraged him to submit his method of treatment to the impartial judgment of his professional brethren.

A week later, he adds, that he had seen the success of his treatment in six further severe cases. In some the pulse had been imperceptible from twelve to fifteen hours, with corresponding unfavourable symptoms, so that he does not hesitate to recommend the remedy strongly.

The rational treatment of cholera must consist in adopting such measures as will rouse the sunken innervation of the ganglionic nervous system, and prevent its deeper prostration. Only thus can we hope to check the rapid progress towards dissolution. As soon as the innervation has become normal, the morbid exosmotic tendency of the blood ceases, and the abdominal secreting organs resume their ordinary functions, together with the peripheric terminations of the capillary system. If this salutary re-action can be obtained without any morbus medicamentarius (as unwelcome salivation, or artificially produced cholera-typhoid,) he does not see why a remedy should not be employed which promises to be beneficial, without such unpleasant consequences.

The valerianic acid exercises a directly stimulating action on the abdominal ganglia, ammonia on the nervous centres and the whole organism. Thus, a combination of both—valerianate of ammonia—offers one of the most diffusable stimulants, which rapidly enters the circulating fluid, to exhibit its volatile effect. In awakening the prostrate nervous power, it corrects, indirectly, the morbidly altered secretions and excretions, and the abnormal blood-krasis. The success of the remedy depends on its methodic employment. It must be administered as early as possible, and without any other pharmaceutical agent. From 3 to 6 scruples were invariably sufficient to subdue the disease.—*Med. Times and Gazette, Dec.* 16, 1854, *p.* 622.

155.—ON THE PATHOLOGY OF SACCHARINE ASSIMILATION.

By Dr. George D. Gibb, &c.

[This is the abstract of a paper read before the Medical Society of London.]

Before entering upon the pathology, the author gave a brief summary of the physiology of the subject to the present time, and referred to the discoveries of Bernard, Pavy, and himself. He gave a table of the pathological conditions of saccharine assimilation, and considered some of the more important. He considered the special saccharine function in diabetes to be paralyzed, and believed no sugar was secreted in the liver in this disease, and in consequence the converting action of the stomach became deranged, permitting of the formation of large quantities of sugar in that organ, secondary to the arrest of the saccharine function in the liver. This view was opposed to that of Bernard, who looks upon the liver as secreting an excess of sugar in diabetes. The principal evidence the author brought forward in support of his own view was the invariable absence of sugar in the liver after death, and the "starved" state of the organ, from its poorness in fat. In fatty liver only did he consider the saccharine function abnormally active. The gorged condition of the portal venous system, found after death, he looked upon as due to hepatic obstruction, from paralysis of the special function of the liver, from taking too much gastric sugar to that organ, which it could not assimilate. If thoracic disease was a frequent termination of diabetes, he believed it was from the want of naturally-formed hepatic sugar to undergo combustion, and not from excessive combustion, as maintained by Bernard. He admitted that diabetes and allied affections were under the governance of the nervous system. He considered an occasional connexion existed between oxalic acid and sugar. Sugar, he said, was one of the great sources of fat in the economy, and he denied that fat was ever re-converted into sugar; there was no evidence to prove it. He confirmed the observations of Dr. Goolden in relation to the sympathy existing between the nervous affections and saccharine assimilation.

[Dr. Gibb chiefly depended on the copper tests, together with the fermentation test by yeast. His copper tests were Trommer's and Barreswil's solution.]

Dr. Garrod said, that with regard to the tests made use of, he thought that the fermentation test, when carefully employed, was the one which gave direct, and the most satisfactory, evidence of the presence of sugar; but it required, for its indications to become manifest a considerable amount of that principle to be present—two grains to the ounce often failing to give an indication. Dr. Garrod felt assured from experience, that, by the different copper tests, many errors were frequently made; and, in fact, sugar may often be present, and not

detected, and again absent, and the indication produced by other bodies. In water, the copper test was one of great delicacy: but by no means so in urine. Dr. Garrod considered that sugar, in small quantities, too minute for the fermentation test, might be most readily detected in urine by treating that fluid with excess of trisacetate of lead, filtering, removing the lead with bicarbonate of soda or potash, and again filtering, by which means a colourless urine was produced, giving, on the addition of a few drops of a concentrated solution of potash, and heat, a bright orange colour, even if not more than a quarter or half a grain of sugar were contained in the ounce, creatine, alkaloids, &c., which might be present, not interfering with this test as with the copper one. By the above modification of Moore's test, Dr. Garrod had been able to detect a trace of sugar, a few hours after meals, in perfectly healthy subjects; whereas that passed at the time most removed from the influence of diet, gave no indication. With regard to the point insisted on by Dr. Gibb, that the liver of diabetic patients after death gave no sugar, Dr. Garrod was able most positively to deny its constant correctness, having, in one case, prepared a considerable specimen from that organ, and in another found its presence. That it might be absent in some cases, when the urine before death had ceased to show the presence of sugar, he would not deny. With regard to the connexion between oxaluria and glucosuria, although fully prepared to acknowledge the occasional presence of sugar in urine containing oxalate of lime, yet he had arrived at the conclusion, after paying considerable attention to the subject, that oxalic acid in the animal body is not produced, as Dr. Prout considered, from the amylaceous or saccharine elements, but was a product of the decomposition of uric acid. Amongst other reasons for this opinion he mentioned the frequent occurrence of mixed urates and oxalates in the urine, the frequent alternations from one to the other in the deposits, the constant occurrence of calculi in which urates or uric acid alternated in the layers with oxalate of lime, the ready production of oxalic acid from the artificial oxidation of uric acid, as by the peroxide of lead, and the probable production of this same acid in the spontaneous decomposition of uric acid in albuminous fluids. Dr. Garrod did not believe that a force capable of breaking up *sugar* into oxalic acid existed in the human body, either in health or disease; whereas a force equal to the disintegration of *uric acid*, and thus forming oxalic acid, was known to be present.

[The following is from Dr Gibb's papers in 'The Lancet:']

Before entering into the pathology, it may be permitted me to say a few words on the physiology of saccharine assimilation.

The *liver* has been shown to be the fountain-head for the formation of sugar, and in health contains a certain quantity of that substance; there it is formed from the blood brought to this organ by the portal vein, and afterwards is carried by the hepatic veins and vena cava

inferior to the right side of the heart, whence it is conveyed by the pulmonary arteries to the lungs, and there undergoes the process of combustion. This fact clearly proves an important connection between the saccharine element and respiration, which time will not permit me to go into; but the experiments of my friend Dr. Pavy show that the presence of fibrine in the blood is essential to the decomposition of the sugar. The formation of sugar in the liver is a process totally independent of saccharine or feculent food, and commences before the birth of the individual, as has been demonstrated by Bernard. The quantity of sugar, however, varies in different classes and tribes of animals, as I have satisfactorily proved on many occasions, being much greater in those whose livers naturally contain much fat, of which the seal and the porpoise may be cited as examples, and also in ducks and geese, and most of the sea-birds. The liver of the cod and other fishes which abound in fat also contain it in large quantity. This fact also, first noticed by myself, shows the strong relationship which fat bears to the presence of sugar, and helps very much towards the elucidation of the pathology of fatty degeneration; in fact, its importance cannot be over-estimated.

Sugar also exists in the serum of healthy arterial blood, in minute quantity, and also in different parts of the venous system, but varying in quantity; thus, Dr. Pavy has found sugar in the jugular veins; it exists in the vena porta but in small quantity, being, however, richer in the former than the latter. The chyle of the thoracic duct contains a little sugar, derived from the lymphatics of the liver, the contents of which acquire a saccharine impregnation, from the saturated condition of the tissue of this organ. The chyle going from the alimentary canal does not contain it. Sugar is an important ingredient in the well-known secretion of the mammary gland, and is familiarly known as sugar of milk; it is present also in the milk secreted in the breast of the newly-born babe, whether male or female. Sugar is also found in the urine of pregnancy, and can be most clearly demonstrated during the process of gestation; and lastly, sugar is found a normal ingredient in the urine of the aged.

Such, then, are the natural or normal states in which sugar exists in the animal economy, and which may conveniently be comprised under the term *Physiology of Saccharine Assimilation*. This classification will permit of our comprehending the states in which the presence of sugar in the fluids and solids of the body constitutes disease, in contradistinction to those which we denominate healthy.

The pathological conditions form a very considerable class, and comprise some of the most incurable diseases to which the human frame is liable. These conditions may be regarded, perhaps, merely as symptoms; but as they indicate constitutional derangements of grave importance, we may, I think, most justly consider them under the head of the 'Pathology of Saccharine Assimilation.'

Diabetes.—That eminent philosopher, Dr. Prout, has satisfactorily

stablisheed that in this disease the primary assimilating organs are unable to assimilate the saccharine principle. In health the *reducing* function of the stomach, on all the forms of the saccharine principle, is accompanied by the development of a *low* sugar, which is speedily *converted* into albuminous and oleaginous matters. The existence of sugar therefore in the stomach is only momentary. Now, in diabetes the *reducing* function of the stomach is morbidly active, while the *converting* function is more or less suspended or paralyzed. It follows therefore that in the diabetic stomach sugar is found in large quantity, particularly when vegetable aliments have been taken; and from the stomach the sugar readily passes into the sanguiferous system. Dr. Prout correctly observes, therefore, that diabetes does not consist, as some have supposed, *in the development of sugar in the stomach, which is a natural process, but in the greater or less destruction of the converting, and consequently of the still more important organizing, functions of the assimilating organs.*

I think it will be admitted that this explanation of one of the first steps in the derangements of this disease is most clearly the truth, and quite incontrovertible. The discovery of sugar in the liver, subsequent to the enunciation of Prout's doctrine, so far as I have investigated the matter, does not appear in any way either to interfere with it or in the slightest degree to set it aside. He has acknowledged, however, that sugar does appear as the result of secondary assimilation in advanced stages of diabetes, but that it never appears in the healthy state of the system during the secondary assimilating processes. The latter part of this assertion is now disproved by Bernard's discoveries. From the very large quantity of sugar thus generated in the stomach, it is a matter for little surprise that the blood should be thoroughly impregnated with it, notwithstanding that this has been denied by some; but the fact is now perfectly established. The blood takes up a large proportion of the sugar, but not all, as the alvine discharges are found to be mixed up with perhaps the more solid and crystallizable portions of it, which may be distinctly seen after the lapse of a few hours. The various emunctories of the body assist in relieving the blood of its surcharged saccharine principle; and hence by the kidneys the urine in large quantities is passed, containing much of the sugar; the saliva contains it; the expectoration, if accompanied with, or independent of, phthisis, is mixed up with it; and, lastly, the perspiration, if there is any, which is but seldom, has been proved to contain it by Simon, Nasse, Magendie, and others, and becomes as much impregnated with it as any other secretion in the body.

It is a matter of great difficulty to obtain a diabetic liver *immediately* after death, but those which I have examined some hours after death did not contain any sugar whatever. At first I doubted the correctness of my experiments. On considering, however, that by the same means I had detected sugar over and over again in other livers,

and the fact of being familiar with the means of recognising sugar, my mind became convinced that the *liver* in diabetes is either free from sugar altogether, or deficient in quantity. This was a most important discovery, and quite opposed to the views entertained by Bernard. Could this be explained by the derangement of the secondary assimilating processes, as a consequence of the portal vein taking so much sugar to the liver already formed, instead of permitting of the process of assimilating it naturally from the blood, during its circulation through the hepatic structure? This was evidently one of the causes. This I thought also in some measure might explain, possibly, why this affection so often terminates in thoracic disease, not from its taking too much sugar for destruction in the lungs, but from its taking too little directly from the liver itself, the liver not having the power, unless in a slight degree, of converting the gastric sugar into that form peculiar to itself, and also into fat.

It may be mentioned here, that I am not unmindful of the experiments of the injection of grape and cane sugar into the general circulation through the jugular veins, and their appearance in the urine; nor of those in which the same sugars have been injected into the portal system, and, undergoing the necessary modifications in the liver, have not been detected in the urine. They clearly point to the natural function of the liver in health, in exclusively absorbing the saccharine principle from without. But when this principle becomes excessive, in such a disease as diabetes, the peculiar function of the liver is more or less paralyzed.

My friend, Professor Beale, of King's College, who has chemically examined several cases of the kidney and liver in diabetes, tells me he could not find a trace of sugar in the liver; and in a valuable paper in the 'Medico-Chirurgical Review' on this subject, he mentions that the liver contained a less quantity of fat than in health—"It appeared to be as it were starved, and its secreting cells seem to manifest a tendency opposite to that of fatty degeneration." My own view is thus confirmed by Dr. Beale, and is quite opposed to that of Bernard in so far as the liver is concerned. He believes that diabetes depends upon the excessive and abnormal production of sugar in the liver, (dependent upon an unusual excitation of the functions of the great sympathetic system of nerves,) whereas the contrary is the fact. If such were the case, we ought to have diabetes as a consequence of fatty disease of the liver, which I have shown elsewhere to contain sugar in the quantity mentioned as occurring in the diabetic liver by Bernard; but with the exception of impregnating the fluid of co-existing dropsies, we do not find it in the urine, and certainly there is no diabetes.

I am disposed, therefore, for the present to consider diabetes as the result of the cessation of the natural function of the secretion of sugar in the structure of the liver, and the consequent derangement fo the primary assimilating processes. The primary assimilating or-

gans are thus influenced in some peculiar way, possibly by an abnormal condition of the bile, secreted from an unsaccharine liver, or possibly by an extension of the nervous influence from the liver, sympathetically affecting the stomach and bowels, and also by the powerful reaction produced by the pancreatic fluid, which contains much fat, as the secreting cells of the pancreas have been found gorged with fat in this disease by Dr. Hyde Salter. The pancreatic fluid in health is the real agent in converting starch into sugar, which is slowly and gradually effected, the sugar being absorbed as it is produced. In diabetes this function may be greatly increased, which the fatty condition of this organ would tend to prove.

These, singly or combined, influence the *converting* function of the stomach, and subsequently secondary assimilation.

Without disputing the correctness of the view entertained by Dr. Bence Jones, that there is a stoppage of the healthy changes in the food during digestion, and which occurs at the sugar,—the starch having become converted into dextrin, and the dextrin into sugar,—the conversion of which into vegetable acid and carbonic acid, he states, does not take place; this view being that of the effects of the disease, my theory is strengthened by the post-mortem appearances, to some extent, which have been noticed by various observers, as well as by myself, meagre though they are. In the numerous examinations made by Prout particularly, he found organic disease of many of the viscera, most generally the lungs, in different individuals; while in others all these organs were observed to be apparently sound. He considered from the diversity in the organs affected, and the occasional absence of *all* organic disease, that the presence of sugar in the system does not depend upon, nor produce visible organic lesion of, any particular organ; but that when organic diseases do exist, they are merely concurrent affections. Functionally speaking, he was induced to believe, from very long attention, that the liver is always deeply involved in diabetes. This is an admission of great importance from such an accurate and experienced observer, and is entitled to great weight, more especially since the discovery of its sugar secreting function.

The most constant diseased appearances noticed by Prout after death have been, he says, rather of a chemico-mechanical nature, than strictly organic. These he comprised under three heads:—First, an enlarged and congested state of the kidneys; secondly, a gorged condition of the veins terminating in the portal system, particularly in the veins of the mesentery, and an unusually dark-coloured and fluid condition of the venous blood throughout the assimilating organs; thirdly, but not so constantly, a vascular state of the mucous membrane of the stomach and upper portion of the alimentary canal.

The first and third are what might be expected, from the nature of the disease; but the second clearly points to hepatic obstruction in the circulation of the blood through the liver, and most probably arising

from the quantity of sugar being carried in the blood to that organ, thus helping to paralyze its proper function.

The evidence furnished to prove that the liver is the great organ at fault in diabetes, subject to the influence of a centre such as the medulla oblongata, is, I think, of much value, and it is a source of satisfaction in my being enabled to state, that if I differ with such an observer as Bernard, it is not as to the seat of the disease, but in the nature of the special function implicated, which, at the same time, however, is the principal key-stone, if allowed the expression, of the disease.

It might be urged against the view I have taken of diabetes, that just before death, the urine ceases to contain sugar, as I have myself witnessed, and that the liver might equally not contain it. Now, this is met by the fact, that sugar in diabetic urine or blood can be detected for any length of time, and so can it in a liver containing it in excess, as the fatty liver, a specimen of which I found to contain a trace of sugar eleven days after removal, although in a state of commencing decomposition. At this moment, some diabetic urine which I obtained from a patient at St. Bartholomew's Hospital, under the care of Dr. Burrows, on December 8th, 1853, contains sugar. This peculiar property of diabetic sugar was first mentioned to me by friend Dr. Pavy, and is not possessed by that artificially produced through experiment on living body. Moreover, if sugar existed in such quantity in the liver as Bernard believes, it ought to be detected with perfect facility even a few hours after death; but such is not the case, it is absent. Since the foregoing was written, at a recent discussion upon this paper, Dr. Pavy mentioned that neither Bernard nor himself had found sugar in the diabetic liver after death, which is a point of some importance to establish; I am prepared to believe, however, in the existence of an occasional and perhaps rare exception to this, as has occurred in the experience of Dr. Garrod.

One of the uses of healthy assimilated sugar in the liver, independent of its combustion in the lungs, is the formation of fat, and at a later day, I have no doubt that the relationship existing between fat and sugar, which has been prominently brought forward by me elsewhere, will yet be proved and established. It is one constantly presenting itself in experiments connected with sugar, and latterly I have come to the conclusion, that it is one of the great sources of the fat in various parts of the body; and further, that sugar is convertible into fat, and fat is not convertible into sugar. This, I may mention, is contrary to a view I had formerly held, in common with others, but which I now hasten to correct. The only argument in favour of the conversion of fat into sugar is the action of pure and recently-formed pancreatic juice, which Bernard says emulsionates fats or oils with the greatest facility; the emulsion remains for a long time, and the fatty bodies soon undergo a fermentation, which separates the acids they contain.

There are some other points of interest, in relation to fat and sugar in diabetes, which help further to prove certain points in its pathology. In a paper by Dr. Beale, in the British and Foreign Medico-Chirurgical Review, on the Chemical Composition and Microscopical Characters of the Liver and Kidneys in Diabetes, especially with reference to the amount of fat present, he found that the kidneys contained a much greater proportion of fatty matter than is present in health, and the liver a lesser quantity than in health. In the healthy kidney, the amount of fat is not quite $4\frac{8}{10}$, whilst in diabetes it is occasionally upwards of $25\frac{3}{10}$. In a given quantity of diabetic kidney, he found the fat exceeds that present in the same quantity of diabetic liver, while in a state of health the *converse* is the case; and on comparing the analyses of two of his cases, it is found that the *healthy* liver contains *more than twice the quantity* present of fatty matter than in the diabetic organ. What is proved by these facts of Dr. Beale's? The circulation of saccharine fluid through the kidneys, depositing fat, as a strictly mechanico-chemical result; and a want of the same fluid, healthily secreted, to circulate through the liver, and hence a notable absence of fat! The pancreas may have much saccharine matter circulating through its structure, from the fact of its secreting cells being gorged with oil globules, as shown by Dr. Hyde Salter.

The sugar-secreting function of the liver, then, is arrested in diabetes; and as sugar is necessary for the purposes of the economy, it is morbidly secreted elsewhere, in the stomach especially, from all kinds of food, in which organ the converting process is in abeyance. Our attention, therefore, in the treatment, must be directed towards removing the condition of the nervous system acting upon the liver, so as to restore its functions, and at the same time the converting the function of the stomach, which had become stopped, secondarily to the cessation of the formation of sugar in the liver.

In relation to nervous derangement, being the first step in the disease, and which reflects so much credit upon the sagacity of Bernard in clearly establishing, I believe the special governing centre to be the medulla oblongata, through its important branches and great sympathetic communications. Bernard considers these phenomena in relation to saccharine urine and diabetes, due to increased activity of the function of the liver, dependent upon an unusual excitation of the functions of the great sympathetic system of nerves. He has proved by experiment that the pneumogastrics act wholly as incident nerves in reference to the saccharine function of the liver. Thus, when they are divided in the neck, to produce artificial diabetes, the current of galvanism must be passed upwards through their central extremities, and not downwards through their peripheral. "The galvanism, acting on the medulla oblongata through the upper extremities of the pneumogastrics, produces an influence which, like that derived from the puncture of the nervous centre itself, is transmitted downwards along the spinal cord, and thence by the splanchnics to the hepatic organ."

In terminating this branch of my paper, it may be mentioned that Drs. Bright, Copland, and Watt, of Nottingham, looked upon diabetes as depending upon derangement of the nervous system, but they did not agree as to what part of it. Dr. Copland fixes the seat of it in the great sympathetic system, and instances are recorded where the sympathetic, pneumogastric, and splanchnic nerves were found very much enlarged in diabetes.

Tuberculosis.—In the fatty liver sugar exists in larger quantity than in health. If the condition of the liver was only found in conjunction with pulmonary affections, we might, indeed, explain the manner in which it arises on physiological grounds, from diminished oxygenation and combustion of carbon due to impeded pulmonary circulation and respiration, a consequence being the accumulation of those elements in the liver which could not be got rid of in the lungs. To some extent, perhaps, this might apply to phthisis, but how can we explain the presence of fatty liver in other diseases, where the lungs are apparently quite sound? Further research and investigation are necessary, before a satisfactory solution can be attempted. We know, from the researches of physiologists, the important bearing sugar possesses in relation to respiration, and the error that some have fallen into in regard to the peculiar saccharine function the liver possesses, being over-exerted, so to speak, in diabetes, which, however, is not proved, and which, as I have endeavoured to show, is actually in abeyance. The knowledge of the fact, that fatty liver contains an excess of sugar, will be the means of helping to work out this problem, and its relation to tuberculosis. This leads me on to the consideration of scrofulous matter, and its connexion with sugar.

In Jan. 1850, I opened a large abscess, situated on the back, below the right scapula, in a female aged 23, the subject of general external scrofula. The fluid withdrawn was of a yellowish colour, specific gravity 1028, inodorous, neutral, and of a creamy consistence. In the course of a chemical examination, I applied the different reagents for testing the presence of *sugar*, when I found that Moore's test and Trommer's test gave positive proof of the presence of a considerable quantity of that substance. Microscopical observation showed the usual characters of tuberculous matter, in the presence of cells filled with granular matter, free granules and fat globules, together with pus and lymph corpuscles. In February, this large abscess, having become again filled, was opened, and exit given to a thick, cream-like fluid, of a dark drab colour, mixed with cheesy particles of the same colour. On examination for sugar, the results were again positive, and the microscope showed a large number of pus corpuscles and scrofulous matter; no blood discs were to be seen.

From these experiments, the suggestion naturally arose, of a possible connexion between sugar and tubercle, and which seemed of sufficient importance to investigate by the most careful research.

The first opportunity was seized to obtain tubercles in a crude and

softened state from the lungs of a phthisical patient, who died, aged forty. Those from the left lung were examined: the crude tubercles were treated in the same way as for examination of the liver, whilst the softened tubercles were boiled, filtered, and then examined. A very small amount of sugar was detected, particularly so in the crude form; but the results were not conclusive, by any means, of the relationship supposed. The case was one especially suited, too, from there being a large fatty liver, which contained a quantity of sugar, as usual. Very recently, through the kindness of my friend Mr. Vertue Edwards, the resident medical officer at the Hospital for Consumption at Brompton, I was enabled to obtain from the body of a female, aged 23, who had succumbed from phthisis, portions of tuberculous lungs, enlarged and tuberculous bronchial glands, the glands of the mesentery in the same condition, and some fatty liver. All these were submitted to a series of the most carefully executed experiments for detecting the presence of sugar, some of them several times; but with the exception of the fatty liver, which, as usual, I found to contain a large quantity of sugar, I did not obtain the least evidence of its presence in the tubercles, either in the glands or lungs. Moore's test, only, indicated the presence of this substance; singly, however, it cannot be relied upon.

Further experiments are still necessary, on other specimens of tubercle, more especially if in a state of softening.

Magendie has stated, there are only two diseases known in which the quantity of sugar in the economy has been proved to be increased—*phthisis* and *glucosuria*. It had long been observed, that the urine, as well as the expectoration, of phthisical patients, is sometimes saccharine; but a distinction, he says, must be made between such cases and those which only become phthisical as a result of diabetes. Bernard declares that the urine, in true phthisis, is not saccharine; but that the quantity found in the blood is much increased. Having observed such increase in the blood of a young girl who had been bled, and whose urine exhibited no sugar, he predicted, although no other sign of phthisis was present, that she would fall a victim to this disease, which she did.

Reynoso again, has detected sugar in the urine of patients affected with tubercle, the sugar being more abundant in those cases in which the malady was in its most advanced stages, and when the inflammatory symptoms were most intense.

In the early stages of incipient phthisis, I have no doubt sugar will be occasionally found in urine. I am not prepared to offer any examples in proof of this; but a case given by Dr. Goolden, in his paper in 'The Lancet' of July last, is one well adapted to direct attention to the subject. The patient was a girl, aged 13 years, suffering from constant headache and pains in various parts of the body; in fact, general neuralgia, but the subject of incipient phthisis, and probably the brain suffering from strumous congestion, as Dr. Goolden very

correctly observed. Here the urine was found to contain an abundance of sugar, which subsequently disappeared under treatment.

Dr. Venables has also observed, that saccharine urine is a very common affection amongst scrofulous children of the poor. I feel persuaded its absence will, by and by, be found the exception, in cases of strumous congestion of the brain, and in hydrocephalus.

I have learnt with much gratification, that it is contemplated, at the Hospital for Consumption at Brompton, to examine the urine of all the phthisical patients in relation to the question of sugar in the urine. Mr. Bird, one of the resident clinical pupils at that hospital, tells me he has lately found sugar in the expectoration of a phthisical patient.

All these facts, in relation to tubercle and diabetic sugar and their connexion with one another, possess features of unusual interest to the anxious inquirer after truth, and, collectively, will prove of service in helping to work out further the pathology of diabetes and allied affections.

It may not be out of place here, to speak of secondary phthisis as a result of diabetes. This is almost one of the invariable terminations of this disease: but it is not constant, as some erroneously suppose. I am disposed to consider the phthisical termination more as the result of the want of sugar for consumption in the lungs, obtained through the ordinary and regular channel of the liver; and hence the cessation of a very important function of the lungs, with a deposit of tubercle. This view is opposed to that of Bernard, who attributes the most serious pulmonary affections to the fatigue experienced by the lungs, in their efforts to decompose the large quantity of sugar which he says is formed in the liver, but which, as I have before stated, is not formed in that organ in true diabetes, one of the special points in its pathology, as I believe, being an arrest of its true sugar secreting function.

Abscesses.—The experiments, already mentioned, in relation to the presence of sugar in scrofulous pus, were not sufficient in themselves to prove that other kinds of pus necessarily contained sugar; but to test the subject further, pus from variously-situated abscesses, and from sinuses, &c., secreting matter, was examined with satisfactory proof of saccharine impregnation; thus showing that sugar was a natural ingredient of pus, no matter what its coloue, nature, consistence, or situation.

To mention a few examples: the pus from a large mammary abscess, which was healthy, laudable pus, contained sugar in small quantity. Pus from a bubo contained sugar. Very fœtid pus from the sac of an abscess over the right malar bone in a girl, contained sugar. A chronic fistula in the left breast of a female, in which blue pus was secreted, containing the cyanuret of iron, contained sugar. Common abscesses, wherever situated, when examined, were found to contain sugar.

The fact was thus positively proved, that sugar is one of the normal

constituents (so to speak) of pus, and it is to its presence that the sweetish taste is due.

Dr. Mason Good, in the second volume of the 'Study of Medicine,' in describing pus, says, "It has a sweetish, mawkish taste, (apparently from its containing sugar,) very different from that of most other secretions. He appears to have been the first author who has *supposed* its presence in this fluid.

Its presence may possibly be due either to the albumen found in pus, which, according to Dr. Wright, contains from 58 to 83 per cent., or to a property possessed by the pyogenetic membrane of secreting it with the components of the pus itself. In a letter addressed to the Academy of Science by M. Pelouze, on 'Various Facts in Chemistry applied to Physiology,' he states that there is sugar in the albumen of the fowl's egg. Pus also contains fatty matters, which may likewise account for its presence. But whether the sugar is formed by the decomposition of the albumen, is secreted from the pyogenetic membrane, or is due to the presence solely of either fat or albumen, I am not prepared to say. The presence, however, of either of the latter, both being proximate principles, is a sufficient proof of its elimination from the body.

I have not, myself, examined pus from diabetic abscesses, but I find that a patient with diabetes, under the care of Dr. Fricke, (U. S. ?) suffered much from abscesses on the shoulder, and backs of his hands. The pus from these always contained sugar.

Diseases of the Nervous System.—From the researches that have already been made into the physiology of saccharine assimilation, more especially in connexion with experiments on the nervous system, we find that a sympathy of no ordinary character exists between the base of the brain and the liver. To some extent this is already borne out by the occurrence of not merely saccharine urine in many diseases of the nervous system, but true diabetes has been a concomitant affection of many of them. When it was discovered that the liver contained sugar, and that its secretion depended upon the integrity of the pneumogastric nerves, it naturally led to the inference that nervous influence exerted a governing power on the chemistry of that organ, which was further confirmed by the experiment of puncturing the floor of the fourth ventricle, which produces artificial diabetes. But this experiment is incomplete until we know whether the secretion of sugar in the liver itself is suspended or increased after its performance. If absent, which I believe it to be, then is the theory which I have ventured to propound of the pathology of diabetes further confirmed. In a careful examination of Bernard's writings, and those of Dr. Pavy, I do not find this point attended to. Very much credit is due to Dr. Goolden, of St. Thomas's Hospital, for the application of these views to the pathology of diabetes in connection with, or in relation to, brain affections, and his observations, as far as they have gone, are entitled to some weight.

These nervous diseases shall now be briefly glanced at:—*Epilepsy.*—The researches of MM. Michea and Alvaro Reynoso were the first to show the presence of sugar in the urine of epileptics after an attack of convulsions: this fact they distinctly proved by a number of experiments, and it has been confirmed by other writers. In my own experience, I have most clearly demonstrated its presence on several occasions, both immediately and for some little time after the attack; and I have, on one occasion, noticed its presence when an attack was threatened, but which passed off. In Dr. Todd's "Clinical Lectures on Paralysis and other Affections of the Nervous System," is a case of epileptic hemiplegia (the 57th), where epileptic attacks immediately followed the cessation of frequent periodical attacks of gout, and which left a state of temporary paralysis of the left side; the urine here was found to contain a small quantity of sugar. In an interesting case embodied in Dr. Goolden's paper in 'The Lancet' of July last, the urine was of the specific gravity of 1022, very turbid, with an excess of lithate of ammonia, acid, and containing a very decided quantity of sugar, which continued for several days: there was a slight tendency to chorea in this case. In the cases I have examined, the specific gravity of the urine has ranged from 1018 to 1025; there was nothing unusual in the quantity secreted, and I have not found its presence beyond two or three days at farthest.

In *Chorea, Paralysis, Neuralgia, Dentition, Concussion of the Brain*, and probably many other nervous affections, sugar will occasionally be found in the urine, and, in some instances, diabetes has been a consequence of the affection, if from an injury. Some of the cases briefly described by Dr. Goolden appear to owe their origin to injuries of the brain; and as these were cured or removed, the diabetes disappeared. A case of diabetes was cured by the supervention of an attack of paralysis. He has found sugar in the urine in a large number of cases of epilepsy, paralysis, and chorea, which has disappeared as the symptoms are relieved by treatment. He states, that severe cases of neuralgia, especially tic douloureux and sciatica are often, but not universally, attended with saccharine urine, which disappears when the nervous affection is relieved; and he gives a good case of general neuralgia in illustration; and also mentions another of facial neuralgia under Dr. Dundas Thomson's care. He likewise speaks of sugar being very common in the urine of children who suffer much from teething.

I am enabled to confirm in a great measure the observations of Dr. Goolden; for I have on several occasions found sugar in the urine of individuals labouring under nervous diseases. It is not at all an uncommon thing to find it in the urine of children suffering from dentition, and very recently I have found that it is occasionally present in the urine of hooping-cough, both in its simple and complicated forms; of the latter I may mention epilepsy, bronchitis, and serious brain disease. Reynoso has found it in the urine of hysteria.

Affections of the Base of the Brain, such as tumours, chronic disease, wounds of the fourth ventricle, injury or division of the pneumogastric nerves, will be accompanied or followed by the presence of saccharine urine. The experiments of Bernard are too well known to require a description; but I would refer to the paper of my friend Dr. Pavy, in the "Guy's Hospital Reports," for several additional and very striking experiments in puncturing the floor of the fourth ventricle in rabbits, and which reflect great credit upon him for the care and patience with which they were so successfully performed. Tumours pressing upon this part of the brain, or disease implicating it, or its immediate vicinity, will produce saccharine urine, I believe, as satisfactorily as if an experiment was performed artificially. The complete division of the pneumogastric nerves arrests the special saccharine function of the liver, as no sugar is found either in the liver itself, or blood going from that organ to the heart and lungs, after its performance. It is found, however, in the urine. A partial division, compression, picking, or injury of the pneumogastrics, will most probably lessen or decrease the amount of sugar in the liver, and produce it in the urine. Their irritation produced by the influence of galvanic currents will equally render the urine saccharine; and it is a curious fact, mentioned by Magendie, that in a recent autopsy of diabetes two dark spots were observed at the place where the part in the fourth ventricle must be penetrated to produce the sugar.

It will appear, then, that some most important connexion exists between the nervous system and the special function of saccharine secretion possessed by the liver, which is clearly proved by numerous experiments, some of which are referred to in a previous part of my paper. It further appears, as shown by Bernard, that if the line of communication between the liver and medulla oblongata is destroyed, either by the division of the splanchnic nerves or by section of the spinal cord as high in the canal as is consistent with the life of the animal, that irritation of the pneumogastrics, by puncture or galvanism, is no longer capable of "stimulating the secretive function of the liver to produce a super-saturation of the system with saccharine matter."—(Dr. Pavy's paper.)

I shall not make any remarks on the latter part of Bernard's view, but shall content myself with observing that the nervous system has under its control the saccharine function of the liver, which seems to be influenced one way or the other by not a very great amount of nervous derangement, and it is a condition which should never be lost sight of in the treatment of any affection accompanied not necessarily with diabetes, but with a saccharine state of the urine.

Diseases of the Respiratory System.—Some observers, and especially M. Alvaro Reynoso, maintain that any cause producing an impediment to healthy respiration, or in any way impairing that function, will produce the presence of sugar in the urine. The theory they give in explanation is, that the combustion of the sugar is arrested in the lungs,

in consequence of the defective action of these organs; it passes into the systemic circulation, and is excreted by the kidneys. To prove the truth of this proposition, M. Reynoso brings forward instances in favour of his view. He has detected sugar in the urine of persons subjected to etherization; in that of rabbits suffocated or drowned; in that of living animals prevented from breathing freely; in that of persons affected with tuberculosis, pleurisy, asthma, and chronic bronchitis. In addition to these, Dr. Garrod has reported a case, in the fifth volume of the "Transactions" of the Pathological Society of acute bronchitis, with the urine saccharine. Dr. Beale found a considerable quantity of sugar in the prune-juice expectoration of a patient suffering from pneumonia, secreted shortly before death. I myself have found it in the urine of simple pertussis, as well as when complicated with bronchitis or other chest disease. And Dr. Bence Jones has found sugar in the urine of a woman under the care of Mr. Cæsar Hawkins, who had been kept for twenty-four hours and more under the influence of chloroform; this has been subsequently confirmed in some instances by Dr. Beale.

Magendie states that it has long been observed that the urine, as well as the expectoration, of phthisical patients is sometimes saccharine. Bernard again declares that the urine in true phthisis is not saccharine, but that the quantity found in the blood is much increased.

The question is of some importance as to the origin of the sugar in the urine in impeded respiration, and deserves careful study. To the opinion of Reynoso I am disposed to attach some weight, in regard to this condition of respiration being the cause of the sugar in the urine; but I cannot altogether agree with Bernard, that there is an increased activity in the sugar secreting organ, with the formation of a quantity of sugar in excess above that for healthy combustion in the lungs; and hence its elimination through other channels. The impeded respiration, however, may act in a two-fold manner—first, in not completely consuming the sugar going to the lungs from the liver, thus passing into the systemic circulation, as Reynoso supposes: and, secondly, in exciting or irritating the pneumogastric nerves, which would, more or less, interfere with the due amount of saccharine matter being secreted in the liver, and thus permit of its generation elsewhere. One of the so-called weighty reasons advanced by Bernard against Reynoso's theory is, that after division of the vagi, although respiration is greatly embarrassed, no sugar appears in the urine; that great physiologist, however, appears to have forgotten that when these are divided the liver ceases to secrete sugar, and respiration becomes embarrassed from the same cause as interferes with this function of the liver; this, therefore, is not a refutation of the important part of Reynoso's theory. Notwithstanding this, however, we will admit that pneumogastric irritation produced by the respiratory disease or obstruction, whatever that may be, acts in the same manner in producing sugar in the urine, as experiments artificially performed.

An observation of Bouchardat, referred to by Dr. Beale, to the effect that, in two cases of slight diabetes, the urine was secreted free from sugar upon causing the patients to inhale oxygen, is said to support the theory of Reynoso. I believe the lungs to be in a debilitated condition in diabetes from the want of naturally secreted hepatic sugar. The inhalation of oxygen may act as a beneficial stimulant, and may assist, in some manner, to restore the proper function of the liver; and this being effected, even for a time, it will cause the diabetes to disappear. As remarked before, the gastric sugar circulating in the general system, cannot be compared to hepatic sugar which is more especially suitable for respiratory purposes.

Cholera.—In this disease, where there seems to be an arrest of the assimilating processes, the secondary especially, we cannot be surprised to meet with sugar in the excretions during the progress of the affection. It has not as yet been proved that the liver especially is not one of the principal organs at fault, as shown by the total arrest of the secretion of bile; without, however, going into a disputed and an unsettled question, it cannot be denied, that the functions of the liver, during the prevalence of the disease, are in abeyance, and if one of these suffer, the others by implication may do so likewise. This will account for the presence of the sugar in the evacuations, and in the perspiration.

M. Doyere was the first to find grape sugar in the perspiration of cholera patients, which was subsequently confirmed by M. Poirson, one of the house-surgeons of the Salpetrière, who found a large quantity of it in an aged woman, who died of the disease, whose skin presented a viscous secretion; a part of that examined was removed from the face.

Magendie has found large quantities of sugar in the evacuations of cholera patients. And M. Vernois mentions that he found sugar more easily in the liver in epidemic cholera, than in the normal state. Can this be due to hepatic venous obstruction from impeded respiration, or to the non-secretion of bile?

Milk.—When the specific gravity of milk is taken, it may determine the richness in cream, but not the amount of sugar present. This latter condition is one of some importance, as bearing upon the pathology of milk, and deserves more attention than has been devoted to it, and so far as my own researches have extended, it appears to have been scarcely noticed, with some few exceptions.

If milk is poor in cream, it will not nourish the child, and will produce simple emaciation; it may contain, however, a sufficiency of sugar, which will compensate to some extent for the absence of the cream, as it is quite capable of conversion into fat during the processes of assimilation.

On the other hand, should there be a plentiful supply of cream, an abundance of sugar, and the external characters of the milk unexceptionable, taken with an apparently healthy condition of the mother,

with the production of great emaciation, copious diaphoresis, and diuresis in the child, we must at once suspect the process of saccharine fermentation within the breast, and the development of infusoria; the microscope is here indispensable. The process is a remarkable example of perverted saccharine assimilation, produced by deranged secretion and nervous excitement.

There are many other pathological conditions of the milk due to faulty saccharine assimilation, which will be considered in a future paper. Lehmann detected sugar in the urine of a puerperal woman, in whom the secretion of milk was suddenly suspended on the fifth day after delivery, a forcible illustration of the connexion between saccharine assimilation and the secretion of milk.

Effects of certain Medicines.—Were not the investigations of M. Reynoso deserving of much credit, from the manner in which they were conducted, in relation to his experiments on the urine, we might almost seem doubtful of the results obtained. It appears that certain medicines, whether absorbed through the lungs in vapour, or through the stomach in substance, possess the power of rendering the urine saccharine, but to be detected only by the most carefully conducted experiments. Ether and chloroform have already been mentioned, when considering diseases of the respiratory system, as occasionally producing this result. In the urine of persons under the influence of the bichloride, iodide, or sulphuret of mercury, the salts of antimony, opium, and narcotics in general, or sulphate of quinine, Reynoso found sugar. Reynoso further states, that dogs which have been submitted to the action of arsenic, lead, or sulphate of iron, excrete sugar in the urine. He has likewise detected sugar in the urine of persons treated with carbonate of iron. Previous to reading an account of M. Reynoso's researches on the presence of sugar in the urine, I had long entertained the belief that over-doses of medicine might produce saccharine urine, but had not myself investigated the subject. One of the substances which I thought especially likely to derange saccharine assimilation was tobacco, when used to excess in smoking and chewing.

An example of the production of diabetes by an over-dose of nitrate of potass, I find recorded in the proceedings of the Academy of Sciences of France, in January, 1849, by Dr. Cardan. A man took three ounces of nitre, instead of Epsom salts, which was followed by great irritation of the intestinal mucous membrane, and abundant diuresis; the irritation subsequently disappeared, but true diabetes remained. The report does not state whether the man was cured.

I shall not venture, at present, to hazard any observations upon what has just been said, as a very wide field has yet to be explored before coming to any satisfactory conclusions; but it would almost appear that this function of the liver is affected by medicinal agents more easily than has been heretofore imagined.—*Lancet, Feb.* 3, *Mar.* 10, 24, and 31, 1855, *pp.* 126, 255, 312, 335.

156.—LITHOTOMY SIMPLIFIED.

By George Allarton, Esq., M.R.C.S., &c.

[This is the title of an interesting pamphlet in which this important operation is attempted to be improved. Mr. Allarton's operation is, in fact, a modification of the Marian, which Mr. Coulson says had been abandoned 100 years ago. Dr. Willis lately attempted to revive it, though with some modifications, in a very interesting monograph, which we noticed some time ago.]

In the *Marian Operation* a grooved staff was introduced and cut upon in the middle line, and various instruments were then employed to dilate the opening into the bladder.

Mr. Coulson here remarks "Dilatation was the theory, laceration the practice; the fact is, that the whole of the canal, from the opening in the bulb to the body of the bladder—that is to say, the membranous portion of the urethra, the prostate and neck of the bladder, are more or less contused, lacerated, and injured in a frightful manner.

Willis remarks on this method "No time was allowed for stretching, and as a consequence the parts were torn." Now, the real truth is, that the parts were not injured by the rapidity of the stretching, but by the nature of the instruments employed to stretch them, as we shall hereafter see. Nevertheless, in spite of this frightful laceration and bruising, the operation was so successful that we find surgeons disposed to recur to it even in our own time.

In the year 1727 Douglas, having observed the capability of distending under a gradual dilatation of the male and female urethra, suggested making an opening through the perineum into the urethra, and then dilating with gentian and other tents, and came to the conclusion that "these artificial fistulas may be dilated so as to extract any stone without cutting the body of the bladder or lacerating any of its parts." In this conclusion, says Dr. Willis, I unreservedly agree.

In 1796 Deschamps proposed a mixed method, but his incision, though directed to the membranous portion of the urethra, was oblique and not in the median line, but he dilated as in the Marian method, but more slowly.

In 1819 Sir Astley Cooper, with Drs. Neil and James Arnott, succeeded in extracting a stone through a fistulous perineal opening, enlarged by the beautiful hydraulic dilator of Dr. Arnott. The dilatation lasted thirty hours—the stone was as large as a small walnut, the diameter of the passage being dilated to three-quarters of an inch in diameter.

In 1826 Monsieur Guerin, of the Bordeaux Hospital, proposed an operation as follows: On the grooved staff an incision was made, commencing at the bulb, and carried down the raphé through the sphincter ani into the rectum. The groove was then entered by the

membranous portion, and a director carried on into the bladder, and the staff withdrawn. The forefinger was now gently pressed into the bladder, where it was retained some time, and the dilatation completed with dried tents pierced with a canula for the escape of the urine.

In 1830 Dr. BUCHANAN, Surgeon to the Royal Infirmary of Glasgow, recommended an operation similar to the one known as Dr. Arnott's method, ignorant that it had already been carried into practice by Sir A. Cooper, at the recommendation of the Arnotts.

In 1840 I performed an operation subsequently described, for the removal of a calculus, by cutting in the median line, and dilatation.

In 1843 a successful operation on DR. ARNOTT'S plan was performed by Mr. Elliott. Another by Dr. Wright, of Malton, and in the same year an unsuccessful case by Mr. Fergusson, of King's College Hospital, upon, if I remember right, an aged man, who died from exhaustion. In 1846 was published in the 'Medico Chirurgical Review' an account of, perhaps, the most successful method of operating that has ever yet been practiced, and as it appears to be scarcely known to the profession, I shall take the liberty of extracting verbatim, the portion of the review which especially treats of it.

Extract from 'Medico-Chirurgical Review:' "This operation is published to the world by an Italian named Joseph Bresciani de Borsa, in an essay on *Theoretical and Practical Surgery*, published at Verona in 1843, and *Theoretical and Practical Observations* published at the same place in 1844."

The Reviewer says, "It is not often that works on Medicine and Surgery by Italian practitioners come into our hands, and, judging from the specimen before us, we regret that we do not see more of them in this country. Dr. Bresciani de Borsa is in large practice in Verona, and is Surgeon to the General and Lying-in Hospitals of that city. The author details at great length the symptoms, modes of detecting, and prognosis of stone in the bladder. After dwelling minutely and ably upon the anatomy of the parts concerned, and describing all the various operations which have been devised for the removal of the calculus, he presents us with an account of his own mode of procedure, which he states to have been attended with an amount of success which seems to us almost incredible, much as we are disposed to rely upon the veracity of so industrious and candid an observer.

"This operation is, in fact, a modification of that of which Signor Manzoni of Verona published an account in 1808; and Dr. De Borsa declares that of one hundred cases operated upon by that gentleman and himself, only one has died, and that from causes irrespective of the operation.

"MANZONI'S operation consisted in cutting into the spongy portion of the urethra only, and then dilating the bulbous and prostatic portion sufficiently with the finger to admit of the introduction of the

forceps and the removal of the stone. Dr. De Borsa, however, judging it more reasonable to cut into the more dilated portion of the urethra, carries his incision from the bulb to the prostate, and even unavoidably scarifies the latter, when it advances more than usually forward. As the admission of such an operation depends upon the dilatability of the prostate being satisfactorily proved, the author advances several reasons for this, founded on the nature of its anatomical structure, and cites various facts observed by himself and others. He then details the various steps of his operation. 'Having placed the patient in the usual position, (it is much preferable to retain him in this by means of assistants than by ligatures, for the mere ceremony of adjusting these, causes a great dread to the patient, which may alone suffice to induce a low and fatal form of fever) introduced the staff, and made a sufficiently large external incision, I open with a small lancet-pointed, double-edged, strong scalpel, the whole of the membranous portion of the urethra, so as to expose the instrument to the extent of about ten lines, in doing which, it may in some cases easily happen that the apex of the prostate is also cut; although, in the case in which the patient died it was found entire. I now take hold of the handle of the staff, and passing my left forefinger into the wound, feel the groove exposed; and as others would pass some sort of gorget through the prostatic portion of the urethra and neck of the bladder, I only introduce my finger into the bladder, being certain that it never can make a false passage, since I keep it in contact with the metallic instrument. I take care, nevertheless, not to pass my finger along the groove because I should then thrust it against the internal or posterior angle of the wound, and then against the great bulk of the prostate.

"'Scarcely do I touch the groove before, instead of following it, I pass my finger upon the right side of the staff (as regards the patient), and carry it quietly and without any obstacle into the bladder. One of the advantages of this modification is that it enables me, in most cases, to come at once in contact with the stone. I then remove the staff, still, however, retaining my finger within the track of the incision, and gently moving it about in a semi-rotary manner, effect a much greater dilatation of the prostatic urethra and neck of the bladder. Next I pass in the forceps behind my finger, and seize the stone.

"'It has to pass along a track of only from twelve to fifteen lines, or even less, since the inverse cone formed by the forceps approximates the neck of the bladder and urethral aperture, for which reason the space is shorter and more easily dilated. I remove the stone with two, or at most three semi-rotations, performed with circumspection and care along an axis, which, commencing at the neck of the bladder should pass through its neck, and following the centre of the prostatic urethra, terminate in the centre of the perineal aperture.

"'I am, in general, not more than a minute, after opening into the urethra, before I have extracted the stone, and the operation has

always succeeded in my hands. Safety, simplicity, and celerity, I have already said are desiderata in every operative process, and I am in a condition to prove that they attach to this one recommended by me. All I employ are simply a bistoury and the forceps, simplicity surely, in comparison with the multiplicity of instruments which has been recommended in the various and numerous modes of performing cystotomia.

"'By passing my finger along the staff, I secure that safety which is not usually a characteristic of operations for stone. It is related that even the celebrated Scarpa passed the gorget, which was looked upon as the palladium of his fame, in between the bladder and the rectum. The same thing has occurred to many, otherwise skilful operators. I am certain of not injuring either the pudendal artery, the prostatic venus plexus (so frequently in a varicose condition in the aged), the body of the bladder, or the rectum. Farther, by the preservation of the whole of the prostatico-vesical canal, inflammations between the rectum and bladder are avoided, inasmuch as urinary infiltration is prevented, in consequence of the prostate not being divided. The rapidity of the operation is shown by the fact, that instantly after I have opened the urethra I have the stone in my hand; and any one who has even once performed this operation will bear witness to my assertion.

"'I may observe, that it seems almost impossible that so many operators, at all periods, have written so much, and cudgelled their brains to invent new operative proceedings for lithotomy, by deep prostatico-cystic incisions, while with a simple urethrotomy the desired end may be obtained, as I can prove by so many cases.'

"The author next enters into a detailed account of the twenty or thirty modifications of the operations of lithotomy which have been at various periods devised, exhibiting the particulars in which his own may be advantageously compared with these, and terminates his essay with a report of the discussion upon the dilatability of the prostatic portion of the urethra, which took place in the medical section of the scientific congress held at Padua two or three years ago. At this, several eminent practitioners expressed doubts of the feasibility and safety of his proceedings; but he truly observed that facts are far beyond theoretical objections. Upon it being insinuated by some that success attendant upon the removal of small stones ought not to confer approval upon a method, De Borsa replied by exhibiting large calculi which he had removed. He stated, however, that neither this nor any other form of operation can be universally practised, and that the bilateral operation of Dupuytren is best adapted for stones of great magnitude, and the hypogastric operation when this is excessive. So, too, when the prostate is much diseased, the hypogastric operation may be more eligible. The dilatability of the prostatic urethra, independently of incision or laceration however, was considered so doubtful by many, that a commission was appointed to con-

duct some experiments on the dead body. However, the commission was formed, and he consented to exhibit the celerity with which the finger and forceps might be introduced into the bladder, but declined allowing the soundness of his practice to depend upon conclusions drawn from circumstances so different from those prevailing in the living body. Here he only exercised a sound discretion. In the two instances experimented upon, on examination afterwards, the prostate was found to be only slightly lacerated to the extent of two or three lines, although the forceps had been introduced, and the stone removed by the aperture.

"This for the dead body; but the author reiterates, that in the living subject, a division of even the apex of the prostate is exceptional and unessential, and that in the only case in which death allowed of an examination being made, the part was found entire, as testified by competent witnesses, although the stone was fourteen lines in diameter.

"He observes, moreover, of those who persist in maintaining that he must lacerate the prostate in dilating it, that they must find great difficulty in explaining Manzoni's great success, who only opened into the spongy portion of the urethra, leaving entire between the end of his incision and the apex of the prostate, the bulb and the membranous portion of the urethra, and extracting the stones without any laceration whatever, and with uniform recoveries.

"The position the author occupies, as one of the élite of the profession, in his own country—his great experience and evident candour, lead us to accept his statements as well grounded, and to hope that they will be put to the test of experiment among ourselves."

I have made this long extract, considering it to be one of great importance, and as also necessary to shew in what respects my operation differs from the one here described.

I may first observe that the ligatures are not at all necessary to control the patient, unless we have a deficiency of assistants. The method I employ is simply this,—I introduce a grooved staff in the usual manner, and of the usual size, and confide it to an assistant, with directions to keep it perpendicular and hooked up against the pubes; I then introduce the index finger of my left hand into the rectum, placing its extremity in contact with the staff, as it occupies the prostate, and press it firmly against the staff, so as to steady it, then, with a sharp-pointed straight knife, with tolerably long and rough handle, I pierce the perineum in the middle line, about half an inch above the anus, or at such distance as may appear necessary to avoid dividing the fibres of the external sphincter,—I carry the knife steadily and firmly on till it strikes the groove of the staff, the deep sphincter lying between the knife and the directing finger, which enables me to judge of the distance as the knife passes along. If the incision be not made exactly in the median line, the contracting fibres

of the injured muscles draw the point of the knife from its direct line and interfere with the accuracy of striking the staff, hence the advantage of the long rough-handled knife, which affords a firmer hold and better purchase. Having struck the groove of the staff, I move the point of the knife along the groove towards the bladder a few lines, and then withdraw it, cutting upwards, so as to leave an external incision of from three-quarters of an inch to one and a half inches, according to the presumed size of the stone—the escape of urine indicates the entrance to the urethra. I then introduce a long ball-pointed probe or wire through the external opening into the groove of the staff and slide it into the bladder, to sufficient depth to insure its safe lodgment in that viscus, and withdraw the staff. I then well grease the index finger of the left hand and pass it along the probe, with a semi-rotary motion, through the prostate into the bladder; which procedure is achieved without difficulty, and when the stone is free it comes at once in contact with the finger, and, if of moderate size, passes at once into the wound on withdrawing the finger, the patient having power to strain upon and thereby facilitate the extraction of the stone; this last-mentioned power being one of the great advantages of this operation; the incision being made strictly in the median line no muscles are divided and the integrity of the bladder being preserved, it is under the control of the patient, who exerts, at the wish of the surgeon, a powerful propulsive effort which keeps the stone in or in contact with the internal extremity of the wound, where it is easily seized by the forceps and extracted by mild persevering traction. Now, as the aperture is necessarily the size of the finger which produces it, if the stone be large some other dilating power must be employed in addition to the dilating effect of the forceps and stone combined; for this purpose Weiss' three-bladed female dilator, Arnott's hydraulic dilator, or, what is at once ready and effective, the addition of the vulcanized india-rubber finger stalls one over another until the finger is sufficiently enlarged for the purpose, the outer covering being well lubricated with lard before being introduced. But Arnott's dilator, where it can be procured, is by far the most efficacious, though not the most expeditious means. Should the stone be of unusual size, it may be readily broken by a short, strong, and straight lithotrite, or by a strong and suitable pair of forceps closed by a screw, if the stone be soft and yielding—I say readily because the stone is, in this operation, within so short a distance of the external aperture that mechanical aid can be brought to bear upon it without the slightest difficulty or risk; again, should the stone resist the efforts to crush or extract it, the wound can be readily enlarged upwards or downwards, by dividing the deep fascia, or even be converted into a bilateral aperture sufficient to extract any average sized stone. I believe the deep fascia to be the great obstacle to the extraction of the stone: I have observed that it acts like a ligature round the finger or forceps, and resists the extraction of the stone.

The patient suffers little in this operation, and merely complains of the pricking-stabbing sensation of the first thrust of the knife, the subsequent extraction of the stone does not appear to cause pain; he passes his urine freely by the urethra as well as by the wound, from the time of the operation, and there can be little doubt that the wound might be nearly healed by the first intention with perfect safety. Two of my patients were up and out the day after the operation, and one was walking out on the third day (a cold, snowy, frosty day.) The wound left entirely to nature, without tents, &c., heals in about three weeks. The patient, from the completion of the operation, excites no anxiety for his safety—he usually sits up and moves about on the following day, and I cannot well imagine the advent of inflammatory or other bad symptoms.

The three principal features of this operation, as differing from De Borsa's, are—First, the introduction of the finger into the rectum as a guide, by which the staff is held steady, and the course of the knife guided, so as not to approach too closely to the rectum and to insure the striking of the groove of the staff on the first atttempt.

Secondly,—The cutting on the staff daggerways, and completing incision at one introduction of the knife.

Thirdly,—The complete withdrawal of the staff on the introduction of a long probe, which renders the chance of lacerating the prostate less, on the forcible introduction of the finger.

The great advantages of this mode of operating can scarcely be estimated, since any surgeon of average skill and anatomical knowledge may undertake the operation with every prospect of success, and with the almost certainty of his patient doing well: it is impossible to miss the bladder, and equally so not to extract the stone. Nothing but constant practice can make perfect, but if there be a great operation, which can be done successfully with little practice, this is the one; to perform it first on the dead subject is no doubt a great advantage, but the operation on the dead is quite a different thing, there is little resistance to the passing of the finger, the sensation which it produces is totally different—there is no expulsive effort of the abdominal muscles, to propel the stone into the wound—there is no blood—no evidence of feeling—yet I would recommend every man, about to operate for the first time, to try his hand upon the dead subject first. Let him learn to construct Arnott's dilator, and estimate the respective powers and advantages of different dilators,—reflect upon the anatomy and relative situation and distances of the parts to be operated upon.

The first operation which I performed for removing a stone on this principle, was fourteen years ago; the calculus was sharp at one extremity, and had become engaged in the neck of the bladder, whereby the flow of urine was arrested, and the patient, a youth of about nine years of age, was suffering the most intense agony. A catheter was introduced which pushed back the calculus, but, impelled by the efforts

to pass urine, it instantly returned to its seat. I passed a staff into the bladder, introduced the forefinger of my left hand into the rectum, and then made an incision in the raphé just above the anus, and carried the knife on into the membranous portion of the urethra—here the extremity of the calculus was touched by the point of the knife, and I made an effort to seize it with a pair of dressing forceps, but it slipped back into the bladder.

I then passed a probe into the bladder, guided by the staff, and withdrew the latter instrument, I then thrust the index finger of my left hand slowly and carefully along the probe into the bladder, when the stone immediately followed the withdrawal of the finger and was readily extracted with the forceps. A flexible catheter was then introduced through the urethra, and secured by means of a ring and tape to the penis. In the course of the night urgent desire to make water caused the little fellow to get up, when the catheter escaped and the urine followed in full stream. The next day my patient was up and about, and he never had difficulty afterwards in passing his water by the natural passage, the wound soon healed, as it was small, and very little urine escaped by it at any time.

The calculus was as large as a large French bean, but elongated and sharp at one end. This case produced a deep impression upon me—the facility with which I pursued the object of my search, the freedom with which the urine subsequently passed, the entire absence of any unfavourable symptom, and the patient getting about at once and without any apparent suffering, convinced me that an operation by dilatation was not only practicable, but free from danger.

In 1849 I was called upon to attend a little boy aged two years and seven months, who had been suffering for some months with symptoms of stone. During the few weeks antecedent to my attendance, he had prolapsus of the rectum to a frightful extent, the gut being ulcerated and inflamed, and covered with particles of grit and dirt. I prescribed full doses of laudanum with a little bicarbonate of potash, which afforded great relief. I was in this case very anxious to try Dr. Arnott's operation. I wrote to Dr. James Arnott, of Brighton, on the subject, but the patient being a violent-tempered child, and residing two miles from my house, I felt that it would involve great trouble and delay in effecting the necessary amount of slow dilatation, although I had provided myself with all the instruments necessary for the operation. On pointing out to the Doctor the difficulties with which I should have to contend, he recommended me to try De Borsa's operation, and kindly referred me to the number of the *Review* containing its description, when I was not a little astonished to find that it was an operation very closely resembling the one I had performed in 1840. The morning for the operation arrived, I determined to try Arnott's dilator, but to employ it rapidly instead of gradually, and was only prevented doing so by discovering that the tube of the dilator had been imperfectly soldered, and consequently permitted the contents to escape. I, there-

fore, commenced the operation which I had before so successfully practised, and completed it in a few seconds, and with astonishing ease.

The child being bound, which in this case was essential, from its violence of temper, I introduced a grooved staff, which my friend, Mr. Hadley of Birmingham, held firmly at a right angle with the body, and hooked steadily underneath the pubis. I then passed the index finger of my left hand into the rectum, when I could readily detect the calculus alongside the staff—I pressed the point of the finger against the staff so as to steady it. I then entered a straight narrow-bladed scalpel about half an inch above the anus, and carried it forward to strike the groove of the staff in the membranous portion of the urethra, but did not succeed in consequence of the shortness of the handle, which prevented me holding it with sufficient firmness. I therefore withdrew my finger from the rectum, and passed it into the wound, feeling for the groove of the staff with the finger-nail, and then opened the membranous portion of the urethra.

I now introduced a long probe-pointed wire through the groove of the staff into the bladder and withdrew the staff, then introduced my finger, previously greased with lard, with a semi-rotary motion into the bladder, where it came at once into contact with the stone, and so powerful were the expulsive efforts that I scarcely needed the forceps to extract the stone, which was an inch and three-eighths long, and seven-eighths of an inch wide.

During the night the urine passed freely through the wound, as well as the urethra.

The prolapsed bowel, which was most troublesome during the operation, had resumed its natural state and had acted freely during the night. The next morning I was both astonished and alarmed to find that my patient was out—in the temporary absence of his mother he got up and crossed the street in his nightdress without any apparent inconvenience—and from this time he continued to run about and play as before the operation. The wound was healed in about three weeks.

The last case upon which I operated at the commencement of the present year, was a young man of twenty years of age, who had been suffering frightfully during the last two years, and latterly had passed large quantities of blood with his urine—he was excessively emaciated, and by no means in a desirable state for the operation, but by opiates and a few days' quietude and careful diet his condition was much ameliorated, and I decided upon removing the stone (a large mulberry calculus). My friend, Mr. Hadley, with Mr. Duncalfe, kindly rendered me assistance, and the operation was performed as the others. I at once struck the groove of the staff, and completed the incision at one introduction of the knife. I had, however, not provided myself with a dilator, and the stone which was an inch and a quarter in diameter, and round, required some little effort to extract it, the forceps increasing its diameter to nearly two inches.

Yet the patient assured me that he suffered only during the stabbing incision, which was momentary, and, as in the previous cases, the urine flowed freely through the urethra from the first, and when the patient pressed his hand on the wound, which he generally did when he wanted to make water, the urine flowed without obstacle by the natural passage. This man was operated upon on the Monday afternoon, and on the Wednesday following, when I called to see him, he was out walking in the frost and snow, and never had an unfavourable symptom—he rapidly acquired flesh, and was quite well and the wound healed in three weeks.

The advantages of this operation are these:—The impossibility of missing the bladder—the smaller amount of cutting than in the lateral operation—the neck of the bladder being uninjured—the smaller amount of blood lost—the prostate being merely dilated not incised—the urine being at once passed by the urethra as well as by the wound, unless union by the first intention be effected—the facility with which the stone is reached, the patient being able to propel it towards the wound—the very short distance between the external opening and the interior of the bladder—the capability of breaking or crushing the stone, and washing out the bladder and freeing it from any minute particles—the small amount of pain—the absence of danger from urinary infiltration—no muscle or vessel of any consequence being divided, no subsequent imperfection can arise—no danger of wounding the rectum—the rapid recovery,—the patient being able to go about the next day—and the great facility with which the operation can be done by any practitioner of ordinary skill and ability.

The recommendations for guidance in the operation are few:—Have your knife straight and narrow bladed, with long handle,—have a good long ball-pointed probe—take care to pare the nail of the dilating finger, so as not to injure the prostate—employ plenty of grease to lubricate the finger—mind that the probe is well in the bladder before you withdraw the staff—and if the stone be large, run a probe-pointed scalpel down the groove of the staff, and cut down towards the rectum through the deep fascia, the rectum still being protected by the finger; or, while your assistant is employing traction and the stone dragging forward the deep fascia, carefully divide with the point of the knife the obstructing margin of fascia downwards towards the rectum, which will enable you to extract the stone more easily—take especial care to have the means of dilating, and instruments for crushing or breaking a large stone—have your forceps as light as possible—take care to wash out or inject the urethra, so that no coagula of blood obstruct the flow of urine—and after the operation place a piece of soft moistened sponge against the perineum to absorb any discharge.

Reflecting upon the unfortunate cases which have so recently and unjustifiably been dragged before the public, I cannot but regret that this novel and safe mode of operating has not been published and circulated sooner amongst the profession, as I flatter myself that its gen-

eral adoption would render the mortality trifling indeed, and put an end to the deplorable misfortunes which will occur occasionally to the best operators. Every surgeon who has performed an important operation must have felt how cruel and unjust is the criticism too frequently indulged in by rival practitioners, and it is but too true, that these criticisms and comments generally proceed from a class of men either incompetent themselves or jealous of the pretensions of their neighbours. That abuses should be enquired into, there can be no doubt, but that an error of judgment or manipulation should be visited by public censure and exposure, is monstrous. First, carefully select the best men for public appointments, then protect them to the uttermost in the exercise of their onerous and responsible duties and let it not be imagined that a man who commits an error of judgment, by which the life of a fellow creature is sacrificed, remains unpunished—the accusing conscience is a far safer and more salutary monitor than the brawling censure of the unfeeling, the unprincipled, and the incapable.

I am quite sure that a safe and easy method of removing stone is a desideratum which can scarcely be appreciated—had we but the correct statistics of lithotomy in hospital and private practice, we should not find men always pursuing the too fatal steps of their predecessors, fearful of the result, yet dreading to deviate from the long beaten but treacherous track.

If, as I have shown, the calculus can be got at so readily in the median line, without dividing muscles, arteries, or other important parts—with the certainty of never missing the bladder, and never dividing the seminal ducts—with the capability of extracting large calculi or readily crushing them, the patient all the time retaining the expelling power of the bladder to aid the efforts to extract the stone—urinating freely by the natural passage—suffering little during the operation, and comparatively nothing after, and able to walk about on the following day; surely we may claim for such an operation the favourable consideration of the profession. That it should have a fair trial, I am most anxious—that it will succeed, I am most sanguine—and that it will be a great boon to sufferers from stone, I most firmly believe.

Having described the mode of performing the operation (see page 421), I shall proceed to explain its anatomical bearings, and what I conceive to be its advantages over the ordinary lateral operation, enumerating at the same time the structures divided in the respective operations.

On taking a survey of the perineal region, we cannot but be struck with the peculiar arrangement of the deep perineal fascia, which is destined to support the weight of the viscera, to resist the powerful action of the abdominal muscles, to give attachment to the perineal muscles, and to close in the great pelvic aperture. This fascia, so important in the median operation, is stretched across the perineum, and divided into two layers, separated above by the

thickness of the pubes, in the middle by the length of the membranous portion of the urethra, and then becomes united at the anus. Between the layers of deep fascia are situated the membranous portion of the urethra, Guthrie's and Wilson's muscles, Cowper's glands, the arteries of the bulb, and part of the internal pudic arteries, but as we cut in the middle line in performing this operation, and the parts above-mentioned are placed on either side of that line, it is obvious that they will not be divided or injured if the operator be careful.

As regards the instruments employed for distending the aperture made into the bladder, we may mention Weiss's two and three-bladed female dilators, which will answer very well where the dilation is not required to be considerable; but no instrument can approach Dr. Arnott's hydraulic dilator,—it is one of the most beautiful applications of physics to surgery that has ever been made,—and so manageable yet so powerful is this instrument, that we can dilate with it suddenly—or, almost imperceptibly, and to any required extent,—at the same time feeling sure that however much the parts are stretched they cannot be bruised, since the pressure is distributed so equally over all the parts submitted to it.—*From "Lithotomy Simplified."* By George Allarton, Esq. London: Ash and Flint, Wellington-street, Southwark.

157.—ON LITHOTOMY.

By James Syme, Esq., Professor of Clinical Surgery in the University of Edinburgh.

[Mr. Syme gives us an excellent lecture on this subject; but before proceeding to the practical part of his lecture, he makes a few remarks on the symptoms of stone.]

If the patient has a frequent desire to pass urine from uneasiness, which is relieved after micturition, then the case is probably one of mere irritability. If the pain occurs during micturition only, there is probably obstruction to the flow of urine, such as is produced by stricture; but if the pain be little or none at the beginning of micturition, increases towards the close of the act, and lasts with severity for a short time after it, then the probability is, that there is a stone in the bladder. There are also other symptoms of more or less importance; but without entering upon them, I must hasten to speak of the operation.

The stone is in this case obviously too large for crushing, which is not expedient, except for very small stones; it must, therefore, be cut out, and how is this to be done? In some respects there is, and long has been, a great approach to unanimity on this subject, but there is a point of the utmost importance, with reference to the safety of the operation, which is still not yet sufficiently understood or recognised by the pro-

fession generally. All in this country are agreed that it is not expedient to cut above the pubes, or to use the recto-vesical operation, and that the lateral operation of Cheselden is the best; all are also agreed that the incision in the skin should be free and large in proportion to the size of the parts, and that its anterior extremity should be situated at the raphé of the perineum; also that any resisting fibres of the levator ani, or other perineal muscles, should be divided, and that the incision of the urethra should not extend further forwards than the middle of the membranous portion. So far, ninety-nine surgeons out of a hundred are agreed; but now comes the difficulty, how far must the incision be carried through the prostate? In order that you may understand this question fully, it will be necessary for us to begin at the beginning, and to take a rapid glance at the history of the subject. The great fundamental error in the operation of lithotomy formerly arose from the notion that incisions through membranous parts were mortal: as the neck of the bladder being a membranous part, it was supposed that to cut it would be to ensure the death of the patient. This prejudice was the foundation of the operation by the "*apparatus major*," as it was called, in which an incision was made as far as the prostate, and then instruments were introduced, in order to dilate the prostatic part of the urethra and neck of the bladder. This, however, was a delusion, for the part admits of dilatation only to a small extent, and if stretched to a greater degree must be torn, so that the operation by the apparatus major was really a process of tearing, except when a very small stone required to be removed. The next step was the sudden and surprising appearance of Frère Jacques, who, throwing aside the details of the tedious process previously used, as John Bell expressed it, thrust his dagger-shaped knife into the patient's hip, with no other guide than a round staff, and extracted stones of the largest size without difficulty or delay. It is true his patients generally died, but some recovered, and this was enough to open the eyes of the profession, and to show that the dread entertained of cutting the neck of the bladder was groundless. The surgeons of France, Holland, and England proceeded to act upon this conviction; but certainly the most celebrated step in advance, at least as we think, was made by that distinguished countryman of ours, if I may so call him, Cheselden, who operated by his lateral method with a success, that, taking all in all, has hardly been surpassed. It might be said that the operation was now perfect, and that nothing more was required. Unfortunately, however, the difficulty was only begun, the question being, what did Cheselden cut? You would say, there could be no difficulty in ascertaining this; as being a good anatomist, he must have been himself aware what parts he divided, and would communicate the fact to others. You will find, however, that eminent commentators differ in their statements as to his incision, and you will have little difficulty in understanding how this happened when you consider that the part is not within reach of sight, so that the operator himself can alone

have any knowledge where the knife is going, and even he can judge but imperfectly at a part beyond the reach of touch. Rau was a very successful lithotomist; he is even said to have operated in 3000 cases without losing a patient, though that is hardly to be credited; but Rau would never tell any one what he cut. Albinus thought he knew from what he had seen as his assistant, and described as Rau's an operation which misled Cheselden, who, having been induced to try it in consequence of Rau's great success, found that he lost two-thirds of his patients, and therefore went back to his own method. It appeared, in fact, that successful lithotomists were lucky enough to get into a knack of making their incisions safely, and that having acquired this power they were able to employ it themselves, but were unable to communicate it with any certainty to others. This appeared very unsatisfactory, and at an early period of my professional career I endeavoured to ascertain with precision, what it was that required to be cut. It was plain that the success of some surgeons and the failure of others could not be due to chance, but that there must be some reason for it; and I had not investigated long before I thought I saw this reason. The prostate is made up for the most part of a texture of no great firmness, and which does not much resent injury. You will see ample evidence of this if you examine the prostate of a person who has been long treated for retention of urine by an unskilful operator. You will probably find numerous small passages in the gland made by forcible thrusts of instruments, yet these tracks are soundly healed, and the patient has been apparently none the worse for them. But it appears that there is one part of the prostate that is not thus tolerant of injury, and this is placed just at its base. If you take a chesnut, and run a bougie through it from the apex to the base, as I have done with the one before you, and hold the bougie as if it were introduced into the bladder, you will be able to form a tolerable idea of the form and position of the prostate gland. It is agreed by all that the incision through it should not be made directly downwards or directly to one side, but midway between these two directions. Now, at the base of the gland, where it joins the neck of the bladder, there is a dense texture, forming a sort of ring around the urethra, represented pretty well in superficial extent by the *hilum* of the chesnut; it is thickest immediately under the mucous membrane, whence it gradually tapers away. This part of the prostate seems endowed with an extraordinary degree of sensibility. If you pass a bougie in a patient who has never had it done before, you may introduce the instrument up to the prostate and a little way into it without producing more than an odd sensation, scarcely amounting to discomfort on the part of the patient; but when you push it on to the neck of the bladder, he perhaps faints and falls at your feet. When a patient affected with stone is going down stairs, he suffers to a very great degree, and this appears to be due to the stone being then thrown on this sensitive part; and in the same way great suffering is experienced

when the calculus is forced upon the neck of the bladder in the empty and contracted condition of that organ. This sensitive part is, as I before remarked, of very dense texture, and it is very tough and unyielding. I have tried careful dilatation of it on the dead subject, but have hardly been able to enlarge the ring to more than a circle of an inch in diameter. There can be no doubt that if this texture be torn the patient will die; if it be extensively lacerated, death will probably occur within two days; and if the injury be of less extent, chronic inflammation will be set up about the neck of the bladder, leading with no less certainty to a fatal termination. If, however, the prostate be divided as far into its substance as this ring extends, the rest of the gland tears very readily, and without any bad consequence, the muscular fibres of the neck of the bladder separate from each other, and the mucous membrane also yields to the dilating force, so that sufficient space can be obtained without difficulty for the extraction of any stone which the outlet of the pelvis will admit. The result, then, of my investigation was that the different success of different surgeons must depend upon this ring being divided sufficiently by some, and not by others. I then inquired into the means used by different surgeons for making the incision in the prostate, and found that all those who were successful used instruments that might ensure the complete division of the texture in question. This odd-looking instrument is the knife used by the late Mr. George Bell, of this city, who operated very extensively and successfully for stone. This, again, is Dubois' knife, and this the "bistourie cachée," which, when properly set, will ensure, like the other two instruments, the necessary division of the prostate. The most curious illustration of this subject is furnished by the knife of M. Le Dran, who is said never to have lost a single case. It is a curious fact, that I at one time contrived an instrument of precisely similar form, and used it until the introduction of chloroform made it no longer necessary; and you may believe, that instead of being sorry to find that I had been anticipated by Le Dran, I was very glad to find the precedent of so distinguished an operator. The knife run along a straight director introduced into the bladder through the wound in the perineum; and the blade, which is two-thirds of an inch in breadth, is sharp only along the oblique anterior edge. I still believe that such an apparatus is the most certain means of dividing the prostate to the proper extent, when the part is withdrawn beyond the reach of the finger, as it may be when the patient is conscious. When the prostate has been divided sufficiently freely, and the part has been properly dilated with the finger, the urine necessarily escapes, and accordingly I never knew a successful operator for stone introduce the forceps till this had occurred. Not long ago, however, I read to my extreme surprise, a recommendation from a lecturer on surgery, to take advantage of the flow of urine, so that the stone might be washed into the grasp of the forceps. Now, I do not hesitate to say, that any surgeon who so conducts his operation

that the urine does not escape till the forceps are introduced, cannot be a successful lithotomist; he may get on pretty well with young patients, but he will lose a large proportion of adults. Mr. Liston, one of the most successful operators for stone, would have been horrified at the idea of introducing the forceps before the urine escaped.

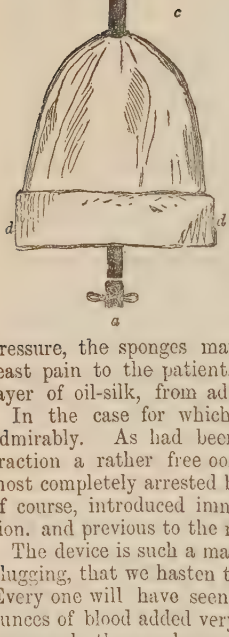
I had long taught the principle I am endeavouring to impress upon you, but had great difficulty in doing so, because Mr. Liston, whose authority was deservedly much respected, had adopted an unhappy way of describing, and a still more unhappy way of representing, the incision through the prostate. In the first three editions of his Practical Surgery, he represents a section of the prostate with an incision, leaving the base entire, and says—"the less that is cut the greater will be the patient's safety." You will remember, the question is not between recklessly extensive incisions, and very limited ones, but between the latter and incisions sufficiently free to ensure the division of the unyielding ring. I had a correspondence with Mr. Liston upon this subject, in which I insisted on the mischievous tendency of the expressions he had used, whilst he, on the other hand, maintained their correctness. At length, in the fourth edition, published only a year before his death, there appeared a paragraph, which I read with very great satisfaction, and which I will now read to you: "I introduce a sketch here originally published some time ago in illustration of some of my surgical lectures in the 'Lancet,' to show what it is indispensable to cut. If that is effected, the dilatation can be carried to any required extent. The two white triangular spaces at the base of the gland, in front and behind, show a section of the dense unyielding fibrous tissue into which the muscular fibres are inserted; this band effectually prevents dilatation or enlargement of the orifice of the bladder beyond a certain and very limited extent, without laceration, dreadful suffering, and imminent danger." This you see completely confirms what I had so long laboured to establish. Entertaining this opinion very strongly, I confess I never felt anything more distressing than the consciousness that I had not cut what I ought to have cut, in consequence of my not being able to reach the part with my finger. When this happened, the patient died; and yet the result was quoted as an argument against a free incision, while I well knew that the error had been of an opposite kind, and it was on this account, that I introduced the knife which I have shown you; but which, I am happy to say, is now no longer necessary, as the patient, when under chloroform, does not make those involuntary muscular efforts by which the parts were, in former times, withdrawn beyond the reach of the operator, unless he had a very long forefinger. If, on introducing your finger to dilate the deep part of the incision, you feel you have not cut enough, pass a straight probe-pointed bistoury along your finger, and, by a gentle sawing motion, carry the incision on to the extent that you find necessary, in order to enable you to dilate with facility. There is only one other point to which I have time to allude

at present, and that is the form of the forceps. It is a curious circumstance, that Cheselden, who, I do not hesitate to say, of all the surgeons of this country, has shewn the greatest surgical genius as an operator, has left no work on surgery, except some remarks and figures of instruments appended to Le Dran's work on the operations in surgery, translated by another man. It is this work that I now hold in my hand; here you see represented, in one of the plates, the operation of amputation by double incision to which I alluded in a late lecture; here is a portrait of his lithotomy forceps, and so accurate was everything he did, that forceps can now be constructed by an instrument-maker from this pattern. Observe the difference between these forceps which I had constructed many years ago, after Cheselden's pattern, and this pair, which I obtained from an instrument shop; the blades of the former are, you observe, considerably longer than those of the other pair, and, therefore, approach nearer to a state of parallelism when the stone is seized, and the extraction is thus rendered much more easy. I may remark, that you will generally see the best operators slow in making their incisions, and rapid in extracting the stone. The incisions require to be made with precision, and dilatation must be thoroughly effected with the finger; and it is no matter how long these parts of the operation occupy; but when they have been properly done, the extraction of the stone is a comparatively easy process.—*Lancet, May* 19, 1855, *p.* 505.

158.—ON THE PREVENTION OF HEMORRHAGE AFTER LITHOTOMY.

By John Hilton, Esq., F.R.S.

A few weeks ago Mr. Hilton had occasion to perform lithotomy on a man who was in a very feeble condition, and to whom the loss of even a small quantity of blood would have been of great consequence. As, however, the man was known to be the subject of an enlarged prostate, and had also suffered from calculus for a very long period, it was deemed likely that the parts about the neck of the bladder would be unusually vascular, and that the bleeding might, consequently, be more than ordinary. With this prospect, it became of importance to devise some means of effectually preventing the hemorrhage which was so much dreaded. To the plan frequently adopted, of plugging the wound with sponge, there are several objections: in the first place, the escape of urine is prevented; in the second, the portions of sponge imbibe the urine, and become irritating to the parts in contact with them; thirdly, they adhere to the sides of the wound, and their removal is attended by difficulty, pain, and risk of exciting fresh bleeding.



To obviate these difficulties, Mr. Hilton contrived the little apparatus which we here figure. It consists of an ordinary tube *a b* (such as those employed after lithotomy by Liston and others), to which is attached, by means of a ring *c*, fixed about an inch and a-half from its extremity, a conical bag of oiled silk, *d d d*. In use, the end *a* is passed up through the wound into the neck of the bladder, and the end *b* being left projecting externally, the cavity of the bag is next filled with sponge, so as to exert pressure on the surrounding parts. In this way the wound may be most effectually plugged, while the free escape of urine is provided for. When it is thought safe, or becomes necessary, to remove the pressure, the sponges may be taken out, one by one, without the least pain to the patient, as they are prevented, by the intervening layer of oil-silk, from adhering to the sides of the wound.

In the case for which it was devised, the expedient answered admirably. As had been expected, there occurred after the extraction a rather free oozing of venous blood, which was, however, most completely arrested by the plan described. The apparatus was, of course, introduced immediately after the completion of the operation. and previous to the removal of the man from the table.

The device is such a manifest improvement over the old method of plugging, that we hasten to attract the attention of lithotomists to it. Every one will have seen cases in which the oozing of even a few ounces of blood added very materially to the patient's danger, and in some such, the employment of the plan described might; perhaps, be the means of preserving life. Should the tendency to hemorrhage be very great, Mr. Hilton suggests that portions of ice might be introduced among the sponge, thus to gain the advantages of both pressure and cold. The apparatus is made by Messrs. Bigg, of St. Thomas's-street.—*Med. Times and Gazette, May* 19, 1855, *p.* 490.

159.—ON THE INJECTION OF IODINE IN OVARIAN DROPSY.

By I. B. Brown, Esq. (Read before the Medical Society.)

The author said that the injection of the tincture of iodine into ovarian cysts was almost new in London practice; not so, however, in Paris or Edinburgh. He mentioned that the injection of the tincture of iodine into the tunica vaginalis for the radical treatment of hydrocele was prominently brought before the profession many years since

by Mr. Ranald Martin, and that it is now the regular practice among English surgeons; but the French surgeons have carried their experimental inquiries much further in this direction. Velpeau first suggested its application for effusion into large joints. Bonnet, of Lyons, however, made the first trial, and with perfect success; this success was soon followed by many others in the hands of Velpeau, Robert, and some others. Then Boinet, of Paris, took up the subject, and applied the injection of iodine into ovarian cysts, and published several successful cases in the 'Gazette Médicale de Paris.' Others followed his example; among the number, Monod, one of whose successful cases Mr. Brown said had fallen under his own observation. The author further said the French surgeons have also used this injection of iodine for peritoneal dropsy, and it appears, from various discussions and trials, with undoubted success. Our intelligent neighbours have gone further, especially Velpeau, Jobert, Maisonneuve, and Ricord, and have injected the peritoneal sac of hernia, and have obtained by it the radical cure of ruptures. Mr. Brown observed that lately his colleague, Mr. Coulson, in St. Mary's Hospital, had tried this plan. The author said that the first real practical application of the injection of iodine into ovarian cysts that he knew of in this country was by Professor Simpson of Edinburgh, who, with his usual zeal and industry, has pushed his acquirements and experiments very freely. The result of his investigations Mr. Brown said he would allude to fully when he gave his own practical remarks. The author then said he would relate a case which he had lately treated in St. Mary's Hospital, and then offer some practical remarks.

Mary B., kindly sent him by Mr. Coulson, was admitted into the Boynton Ward on the 9th of December, 1854. She stated she had had two children, and one miscarriage. She was 27 years of age at the birth of the first, and 29 at the birth of the second child. From the latter period, she had noticed herself getting bigger around the waist, and troubled much by flatulency; she has been regular in menstruation all through her illness; she has no pain, but suffers considerable inconvenience from leucorrhœa. Mr. Brown examined her on the 12th of December, and found a well-marked ovarian cyst, apparently unilocular, and fluctuation distinct; the measurement around the abdomen was twenty-nine inches below the umbilicus, and twenty-eight above. He placed her under medical treatment, with a view to improve her general health, which was much impaired by the secretion of the fluid into the cystic cavity. On the 20th Mr. Brown proceeded to empty the sac. First placing the patient in the horizontal position, he then introduced a large trocar through the semilunar line, and evacuated twenty pints of a thin, turbid fluid, which was found to be strongly albuminous—almost solidified by the joint application of heat and nitric acid; it also contained abundant crystals of cholesterine.

Mr. Brown then introduced a long, flexible catheter, and through

it injected five ounces of the tincture of iodine (of the Edinburgh Pharmacopœia), which is about double the strength of ours. The pain experienced was very trifling, described by the patient as merely smarting. The wound having been closed by strapping, he applied appropriate pads, and one of his many-tailed bandages. The patient was then placed in bed, two grains of opium given, and four ounces of port wine ordered for the next twenty-four hours. In the evening, the patient felt very comfortable, and had no pain or tenderness in her abdomen, only a nasty taste in her mouth, like sea-weed; her breath smelt of iodine. The amount of urine voided for the first two days was more than the fluid taken, but afterwards less. Mr. Brown then ordered a diuretic mixture, and the effect on the secretion was, that the amount of the fluid taken corresponded to the amount voided. There was now an apparent refilling of the cyst, but it proceeded very slowly; the patient's appetite was good; she slept well, and felt no pain.

Jan. 20th, 1855. There appeared about two quarts of fluid in the cyst, but it did not seem to increase, and the patient was decidedly better in health. Mr. Brown then ordered her to wear one of his ovarian bandages, to keep up gentle pressure over the whole abdomen, so as to give support to the whole parietes, and to arrest the refilling of the cyst. In a few days she left the hospital, considering herself much improved, and showing no external signs of the disease. Mr. Brown said he had lately examined her, and could find no increase of fluid, but great improvement in her general health, and she says she is in excellent health and spirits.

The author then said, before offering any practical remarks, he was desirous of drawing the attention of the Society to the important fact of the iodine being taken up into the system, as evinced by the breath and taste in the mouth. He was anxious to ascertain whether it was to be found in the urine, and therefore requested the dispenser, Mr. Copney, to examine some for him on the day after the injection. The urine gave unmistakable evidence of the presence of iodine by the following tests:—

1. The formation of the blue iodide of starch.

2. By its forming, with the salts of lead and mercury, the characteristic iodides of those bodies.

3. By the production of the iodide itself.

Three days afterwards he examined the sweat, but found no trace of iodine; but he thought that if he had tried it sooner he would have found evidence of elimination. Mr. Brown then said, the first question, in a practical point of view, was, Had the treatment in this case been successful? The answer was, that it had been only partially so at present; for although it had not prevented some return of that fluid, still it had evidently controlled the refilling of the cyst, and the general health had much improved. Further time was required to test the extent of benefit. Mr. Brown's opinion was, that in such a

ease the injection of iodine was not likely to effect a complete cure, yet that it would arrest the refilling. He believed that those cases which were radically cured by this plan were the more simple form of unilocular ovarian dropsy—*i. e.*, where the cyst is thin, and its contents non-albuminous, or slightly so. The dropsy of the broad ligament was especially favourable to this plan; but then so it was for simple tapping and pressure, and the latter plan was less hazardous than the former. Mr. Brown observed that we could not expect to solve the whole question until we knew more of the pathological structures of the different cysts in each form of the disease. He stated that he was investigating this question by the aid of the microscope, and hoped to arrive at some certain facts as to the peculiar condition of the secreting surfaces, and peculiar fluid from those surfaces, so that by drawing off a small quantity of the fluid we might be so certain of our diagnosis as to recommend with greater confidence the adoption of any mode of treatment best fitted for the especial case. Mr. Brown said in the next case he would use ten or twelve ounces, so as to insure the complete covering of all the cystic surface.

Mr. Brown rather attributed the partial failure to the circumstance of the whole of the surface of the cyst not having been covered.—*Med. Times and Gazette, April* 28, 1855, *p.* 421.

160.—ON EXCISION OF THE KNEE-JOINT.

By James Syme, Esq., Prof. of Surgery in the University of Edinburgh.

[It is well known that Mr. Syme does not altogether approve of this operation. The profession will see his reasons stated as follow:—]

In the first place, the knee-joint is much larger than the elbow, and its excision is, therefore, more formidable. Secondly, it is a well-established fact, that injuries of the inferior extremity are much more serious in their effects than those of the superior. Thirdly, the leg is merely a support for the body, while the arm carries the hand, by means of which all the undertakings connected with the intelligence of man are carried out. Indeed, some people have gone so far as to attribute his superiority over the lower animals solely to the possession of a hand; although this is an opinion to which it is hardly necessary to say I do not subscribe. The loss of a leg may be supplied by an artificial substitute; but no effort of human skill has ever succeeded in constructing a hand at all comparable to the original member. Last year I amputated the hand in two cases, in both of which Mr. Gray, of London—a very accomplished artist—made artificial limbs, which may be considered as perfect specimens of what the art of man can provide under such circumstances; yet all that can be said of these substitutes is, that they have the shape of a natural hand, and that they possess fingers which may be moved by the other hand; but, for any useful purpose, the instrument to be used—such as a fork, or a hair-brush—

must be fixed into a socket in the palm. So, even if the motion of the elbow were not perfect, the mere preservation of the hand would be of infinitely greater consequence than saving the leg. From these considerations I have long been persuaded that excision of the knee-joint is not an expedient operation; and experience, I must say, tends to confirm this conviction. For, in the first place, it appears that there is much more danger connected with excision of the knee-joint than with amputation of the thigh. In the second place, the cure is far more protracted, tedious, and troublesome. Thirdly, the result is very apt to be unsatisfactory. And, lastly, the limb, in its most favourable condition, must require so much careful attention to protect it against the effects of injury and exertion, as, in my opinion, to be less useful than an artificial substitute.

With regard to the first of these propositions, that regarding danger, although statistics have been quoted to show that the operation is not very dangerous, I could lay before you some facts that would lead to a different conclusion; for all the cases operated on have not been published. But I shall pass from that, and allude only to published cases, and you will find from the reports that even in patients apparently most favourable for the operation, it has frequently proved fatal. And if the mortality, as shown by these statistics, were not greater than that of amputation of the thigh, yet you would have to remember that the operation has been performed only in selected cases, while the results of amputation are collected from all sorts, no matter how unfavourable. Yet of the last twenty cases in which I have amputated the thigh in this hospital for chronic disease, not one has died, although some of them were almost hopeless at the time of the operation, on account of the extreme degree to which they were prostrated by long continued discharge, or other causes. With regard to the time and trouble spent in the after treatment, you will find that eight, ten, twelve, or fourteen months of constant care have been devoted to these patients. Some cases which recently occurred in this hospital, and which are considered very favourable, remained here many, many months, with a nurse especially devoted to them, and made the subjects of a degree of attention and care, which, independently of every other consideration, render this treatment, in my opinion, quite unsuitable for hospital practice. You must also consider the risk the patient runs from this protracted stay in an hospital; for the longer he remains with suppurating sores, the more is he, as well as his neighbours, apt to suffer from vitiation of the atmosphere. Then there is the unsatisfactory result. It has been found that there is great difficulty in preventing the bones from being drawn out of the straight line in one direction or another. Sir P. Crampton operated on two cases, of which one died, while the other made a much boasted recovery, as the patient, to use her own expression, "was able to walk the length of a day." In testimony of her gratitude, she bequeathed her limb to Sir P. Crampton, who divided the bones longitudinally, and presented

one half to the College of Surgeons of England, where you will see that they have been anchylosed at a right angle, which, together with the necessary shortening of the limb, must have rendered her mode of progression rather peculiar in its character. Finally, an artificial substitute is, in my opinion, preferable to the resulting limb; for a labouring man, who could not afford to procure anything but a wooden leg, this would be much more serviceable than the shortened limb, in which exposure to changes of temperature and hard work will be apt to set up disturbance, incapacitating him for exertion; while a rich man, who can afford to procure a shapely and flexible artificial limb, would find it preferable to the shortened and stiffened one resulting from the operation, but this I would not insist much upon, as patients are apt to prefer a part of their own body to any substitute, even though the latter be much more useful. But I must say that at one period I entertained a different opinion of this operation, and I entered upon the consideration of the subject prejudiced rather in favour of the procedure than against it, having been the first surgeon in Scotland to adopt it. Of the cases on which I operated, one died, but the other was very successful. It has been alleged by Mr. Fergusson, formerly of Edinburgh, now in London, that in this case "the patient was obliged to support himself by a crutch, whilst the limb hung useless, being shorter than the other by several inches, and so flexible at the cicatrix, that it was totally unfit either for support or progression." I performed the operation in 1829, and in 1831, in my treatise on "Excision of Diseased Joints," stated that the patient could walk and run; and in 1834, at the first meeting of the British Association in this city, I produced the boy, who there stood, and walked, and ran upon a firm limb, without support of any kind. How the statement to which I have alluded can be reconciled with these facts, I leave Mr. Fergusson to explain, This case, therefore, was a very favourable one; but two considerations afterwards led me to entertain a different opinion of the advantage gained. The first of these was, that as the boy grew, the leg did not lengthen proportionally to the rest of the body, so that he required the sole of his shoe to be four inches thick; but the principal objection was one derived from my experience of excision of the elbow-joint, in which I remarked that it was often long before the oozing of matter ceased from openings in the soft parts. Discharge will often occur in small quantity from an elbow for eight or twelve months after excision of the joint, but in the meantime the hand and arm are as useful, or nearly so, as if this were not the case; but it appeared to me that from the large size of the parts concerned in the knee-joint, the discharge of matter might be expected to prove much more troublesome and protracted, while, at the same time, so long as the smallest sinus remained open, the limb would be of no use for support of the body. I recollect once making an experiment in this theatre with a patient, on whom I had performed excision of the elbow-joint about six weeks before. I desired him to walk on all fours; but though the arm was

already nearly perfect for the use of the hand, the pressure of the osseous surfaces against each other was so painful as to prevent his leaning his weight upon it. Unless the knee be fit to support the body, it is of course worse than useless. On the whole, therefore, while I do not deny that recovery may occur after excision of the knee-joint, and that even, under peculiar circumstances, a limb of considerable utility may be obtained, I have no hesitation in giving the preference to amputation of the thigh. Professing to practice surgery as a useful art, you must consider what course is the best for your patients in general, and not expose the majority to risk for the chance of benefiting individuals, or performing with success a curious experiment. In any given number of cases of incurable disease of the knee-joint, I believe you would find amputation of the thigh productive of a much more satisfactory result on the whole than excision of the knee-joint, and under this conviction I consider it my duty to express a decided opinion against the latter operation.—*Lancet, April* 21, 1855, *p.* 397.

161.—ON POISONING BY STRYCHNIA.—ANIMAL CHARCOAL AN ANTIDOTE.

By Walter Chippendale, Esq.

[In a paper read before the Abernethian Society, the author relates the case of a strong muscular man, aged 26, who had taken four grains of strychnia, with an equal quantity of morphia, dissolved in an ounce of spirit. In rather more than half an hour violent spasms, and convulsions came on attended with extreme pain. When admitted he had lost all command over the lower extremities, the countenance was flushed, expressive of the greatest distress, the tongue was dry, and he complained of great thirst. The tube of the stomach pump was introduced, and three or four ounces of animal charcoal injected, mixed with water, the stomach was then completely emptied. He continued very ill for some time afterwards; the paroxysms were frequent and violent, the pulse 150. Chloric ether and brandy were given, and he rapidly improved, the convulsions ceased, he passed a good night, awoke free from convulsions, and was discharged the next day.]

On reviewing this case we cannot fail to remark the length of time that elapsed, between taking the poison and the first manifestation of the symptoms. This will appear the more striking, if we contrast the period that intervened with the results of those experiments recorded by Christison and other observers, where much smaller doses were employed. The former succeeded in destroying a dog in two minutes, by injecting one-sixth of a grain into the chest, and here the symptoms commenced in forty-five seconds; and Pelletier has seen them appear in so short a time as fifteen seconds. In the above case, to the combination of circumstances unfavourable to the speedy action of the poison, may be attributed the retardation of its effects.

In the first place, strychnia is most difficult of solution—(and the rapid action of a medicine is always proportionate to its solubility); 2ndly, it was in combination with an equal quantity of morphia, the principle of a drug which arrests all vital processes; and, 3rdly, the patient's system had been fortified by habit against this alkaloid, and had in a great measure learnt to tolerate it.

It may excite surprise that the system did not appear to be at all affected by the morphia, which was given simultaneously, and in the same proportion, as the strychnia. This alkaloid, which exerts a directly contrary influence to the latter, may have contributed to the delay of its operation; but we may, I think, more satisfactorily explain the absence of any indication of its action by a reference to the analogous effects of opium in tetanus. We find that in that disease it is most difficult and almost impossible to bring the system under its influence, and fabulous doses have been given without producing any good or ill effect. It might be worth while to try the converse effect of strychnia in hopeless and irremediable cases of poisoning by opium.

Having thus attempted to account for the long interval that elapsed between the introduction of the poison and the first manifestations of its symptoms, a few words must be permitted respecting the plan of treatment pursued. And in this respect the case is especially interesting, in that it demonstrates the correctness of Dr. Garrod's views as to the capability of animal charcoal to combine with and render inert the poisonous principles of animal and vegetable substances, and overcomes the objections that have been raised against its employment.

The decolorizing and purifying properties of animal charcoal have long been known, and it is largely employed in various pharmaceutical processes. The superiority of animal over vegetable charcoal for this purpose is referred to the minute separation of the carbonaceous particles by the inorganic constituents of the substances employed in its manufacture; for its decolorizing power is greatly reduced by dissolving out the salts of lime by an acid. It has been found, in the purification of many pharmaceutical preparations, that the effect of animal charcoal is not confined to the removal of colouring matters, but that it also separates a large proportion of their active principles, resins, &c.; so that a considerable loss is always entailed upon the manufacturer in the purifications of the alkaloids. It is capable of overcoming chemical affinities of some intensity, and will remove lime from lime-water, iodine from iodide of potassium, sulphuretted hydrogen from a solution, soluble salts of lead, and metallic oxides dissolved in ammonia. Hence this substance is a very general antidote in cases of poisoning. The matters thus separated are not decomposed, or altered in their nature, but simply adhere to the surface of the charcoal.

In all those experiments, which were made by Dr. Garrod, when the strychnia was administered, previously mixed with charcoal, the animals were not in the slightest degree affected, and he stated that in the case of the milder vegetable poisons, when charcoal was given within ten or fifteen minutes afterwards, the results were mostly satisfactory. I believe that this is the first instance on record in which animal charcoal has been tried on a human subject. I have searched through the works of the chief authorities on Toxicology, and have not met with a similar experiment, not even on an animal, when so long an interval has elapsed, or so large a quantity of the poison has been given as in the present instance, and I think that the result is so satisfactory as to establish its recognition as a most suitable remedy in poisoning by the alkaloids.

I may add that Dr. Garrod's experiments were not confined to strychnia, but that he found animal charcoal equally efficacious as an antidote for all the vegetable poisons, as well as for hydrocyanic acid. He was induced by the success obtained in these experiments to try its efficacy in poisoning by mineral substances, and he found that, in many instances, when administered in large quantity it was more successful than those remedies usually employed. It was certainly observed to lessen the effects of corrosive sublimate and also of arsenic; but it did not neutralize the operation of these substances so completely as to justify an entire reliance upon it alone.

The beneficial effect of charcoal in these cases has been supposed to be purely mechanical, and that it preserves life by enveloping and isolating the poisonous particles, preventing them from coming into contact with the absorbents, the orifices of which it is believed to obstruct. Such an explanation in the case of the inorganic poisons is doubtless correct, but in the case of the alkaloids we may attribute its efficacy, in some degree, to its power of overcoming chemical combination, and, as in the present instance, of reducing the poison to a condition of inactivity, from its greater difficulty of solution in the uncombined state.—*Med. Times and Gazette, April* 28, 1855, *p.* 423.

162.—ON CHARCOAL FOR SANITARY PURPOSES.

By John Stenhouse, LL.D., F.R.S.

The following table exhibits the amount of the more important gases absorbed by a single volume of box-wood charcoal, as determined by the experiments of Saussure:—

	Volumes.		Volumes.
Ammonia	90	Bicarburetted hydrogen	35
Hydrochloric acid	85	Carbonic oxide	94
Sulphurous acid	65	Oxygen	92
Sulphuretted hydrogen	55	Nitrogen	75
Nitrous oxide	40	Carburetted hydrogen	5
Carbonic acid	35	Hydrogen	17

This table exhibits the extraordinary power of absorption for gases possessed by vegetable charcoal.

The next table, constructed from experiments made by Dr. Stenhouse, shows the comparative power of the three different varieties of charcoal. Half a gramme (five decigrammes) of the following kinds of charcoal absorb the undermentioned number of centimetres of different gases:—

——	Ammonia.	Hydrochloric Acid.	Sulphuretted Hydrogen.	Carbonic Acid.	Oxygen.	Sulphurous Acid.
Wood	98·5	45·0	30·0	14·0	·8	32·5
Peat.....	96·0	60·0	28·5	10·0	·6	27·5
Animal.....	43·5	—	9·0	5·0	·5	17·5

It thus appears that wood charcoal possesses rather the highest absorbent power for ammonia, sulphuretted hydrogen, and sulphurous acid; while animal charcoal is decidedly inferior to both wood and peat charcoal, as an absorber of gases, but as a deodorizer it is of course very greatly superior.

Now, the deodorizing properties of charcoal arise in some measure from its great absorbent power, and which, again, is dependent upon its extreme porosity. The following experiment, made by Mr. Turnbull, of Glasgow, illustrates forcibly the deodorizing power of charcoal. The bodies of two dogs were placed in a wooden box, on a layer of charcoal powder of a few inches in depth, and covered over with a quantity of the same material. Although the box was quite open, no effluvia were ever perceptible, and on examining the bodies of the animals at the end of six months, they were found to be in a very advanced state of decay. The charcoal which surrounded the dogs, on being analyzed, was found to contain appreciable quantities of nitric and sulphuric acids and phosphate of lime, and but little ammonia, and scarcely a trace of sulphuretted hydrogen.

It is commonly believed, and it is stated in many chemical works, that charcoal is antiseptic. This is the very reverse of the fact, as shown by the condition of the bodies of animals which have been long buried in charcoal, which are usually in an advanced stage of decay. This opinion has doubtless arisen from the fact that the ordinary evidences of decomposition—viz., disagreeable effluvia, are absent when bodies pass to decay in contact with charcoal, the charcoal absorbing these gases as fast as they are formed.

There is good reason for believing that charcoal is not only an energetic absorber of gases, but that it is also a powerful oxidizer of them, and that its beneficial operation in many cases arises as much from this as from its absorbent properties.

Dr. Stenhouse very properly points out that the oxidizing property of charcoal renders it improper that it should be mixed with manure, since it would destroy some of those products of decomposition, upon which the fertilizing power of manure is dependent, and so tend greatly to reduce the value of the manure itself.

"Notwithstanding this obvious objection," writes Dr. Stenhouse, "this is the very process which a company, called the 'Health of Towns Improvement Institute,' established in Dublin, is constantly recommending."

Notwithstanding the absorbent powers of charcoal, some have doubted whether it really is a disinfectant or not. This opinion has arisen probably from imperfect views of its *modus operandi*. Since charcoal not only absorbs and destroys all offensive effluvia, and oxidizes many of the products of decomposition, there is scarcely a reasonable ground of doubt remaining, that it does really possess the property of a true disinfectant, acting by destroying those lethal organic compounds upon which infection depends.

The sanitary applications of which charcoal is susceptible, are many and important. The following is an enumeration of the principal:—To the construction of respirators, both to warm the air and to disinfect it, as well as to purify the breath; to ventilation, to purify the air; to prevent the escape of various effluvia from graveyards, and from dead and decomposing animal and vegetable matter, wherever or under whatever circumstances they may be met with. Thus it may be used with good effect in coffins, in the wards of hospitals, in dead-houses, in dissecting-rooms, in malarious districts, and, lastly, on the field of battle, where the work of disinfection is now left to the dog and the vulture, by which the process is far less efficiently performed.—*From a Review of Dr. Stenhouse's Pamphlet, Lancet, April 28, 1855, p. 436.*

163.—CASES OF CHOREA TREATED BY THE CARBONATE OF IRON.

By Dr. Peacock, St. Thomas's Hospital.

[Severe cases of chorea have lately been very common. The following show the superiority of iron to zinc in their treatment:—]

Margaret G., aged 15, was admitted on Feb. 20th, 1855, she had then had chorea for a month, though not very severe, but her extremities and head were in constant motion. After taking an aperient she was directed to have two grains of the sulphate of zinc in an ounce of infusion of valerian three times daily, and this dose was gradually increased by the 17th of March to sixteen grains three times a-day, and she had the tepid shower-bath, first every other day and then every day. The zinc never produced any inconvenience except occasionally a slight feeling of sickliness when first the dose was increased. On the 24th of March no amendment had taken place, the symptoms

being on the contrary more severe. The zinc was therefore discontinued, and instead of it a scruple of the saccharine carbonate of iron was taken three times a-day, the shower-bath being still continued. On the first visit after this change an obvious improvement was perceptible, and from this time she steadily progressed, and was discharged entirely cured on the 5th of April. When first the iron was taken, an occasional aperient was required, but afterwards the bowels acted regularly without any assistance.

Ann B., aged 10, was admitted into St. Thomas's Hospital, under the care of Dr. Peacock, on the 5th of February. Her mother stated that three months before that time she was observed to have singular motions and twitchings in the lower extremities, and this had continued up to two weeks before her admission into the hospital, when the upper extremities also became affected. On the first of February she had a fall, and, though not much hurt, she was alarmed, and the spasmodic affection became much more marked. When admitted there were incessant movements of the extremities, head, and feet, so that she was altogether incapable of walking, or, indeed, of leaving her bed, or of retaining anything in her hands; she could only protrude the tongue suddenly and with great effort, and after several ineffectual trials, and she as suddenly retracted it. The pupils were very much dilated, and the expression of countenance fatuous. She was directed to take an aperient, and was then prescribed 2 grains of sulphate of zinc in ℥ss. of infusion of valerian, three times daily. At first she improved in some degree under this treatment, but the amendment was not maintained; and on the 20th, when she was taking 6 grains of the sulphate of zinc three times a day, and using the tepid shower-bath every day, she had certainly not advanced, and was, if anything, worse than when the treatment was first commenced. Doses of 10 grains of the saccharine carbonate of iron were, therefore, substituted for the zinc. On the 23rd there was some amendment, and from this time she daily improved, till, at the end of the month, she was nearly well, and when discharged, on the 16th of April, she had been quite well for several days. The pupils had recovered their proper size, and her expression of countenance was intelligent and cheerful. At the commencement of the treatment a small blister was applied to the upper dorsal region, but without any advantage.

Remarks.—These cases are not adduced under the most remote impression that they imply the general superior efficacy of the carbonate of iron over the preparations of zinc. The rule in therapeutics which they chiefly illustrate is the propriety of making a change of remedies when, after a fair trial, the one first selected does not appear to benefit. In no disease is this rule more necessary to be observed than in chorea. Cases must have come under the notice of all physicians in which, after iron had failed zinc succeeded, and in which, after perhaps both of these had been tried in vain, the ammonio-sulphate of copper, some preparation of arsenic, or perhaps the addition of valerian to the zinc at once

arrested the disease. Our art is fortunate enough to possess at least half a dozen mineral preparations of proven specific power against chorea, but as yet few, if any, trustworthy rules have been established for our guidance in preferring one to the other. A more minute investigation of the disease might be expected to show that the cases, such as those just quoted, in which the sulphate of zinc manifests not the least efficiency differ in somewhat from those in which it cures like a charm. In our present ignorance, however, we must be content to be guided by the knowledge obtained during the progress of the treatment and to ring changes on the remedies until the right one is found. Perhaps, however, in virtue of their blood-improving qualities the preparations of iron have a claim to be tried before most of the others, and of those preparations the saccharine carbonate, as used by Dr. Peacock, is probably the best for the purpose.—*Med. Times and Gazette, April* 28, 1855, *p.* 411.

164.—*Formula for the Administration of Elaterium.*—The following prescription, copied from the Pharmacopœia of the London Hospital, is a very convenient one for the administration of elaterium :—

℞. Elaterii extract. gr. iss.; pulv. capsici. gr. vi.; hydr. chlorid. gr. xij.; ext. gentian. ℥ss.; sacch. fæcis q. s. Ft. pil. xij. Sit dosis pil. i. vel ij.

If needful, the proportion of elaterium may be increased to 2 or 3 grains, according to the wishes of the prescriber. The capsicum is of the greatest use in preventing the distressing nausea which elaterium often causes.—*Med. Times and Gazette, April* 28, 1855, *p.* 412.

165.—ON STRICTURE OF THE RECTUM.

By Jas. Syme, Esq., Prof. of Surgery in the University of Edinburgh.

The rectum is liable to stricture of two kinds; the one a mere simple induration of the coats of the bowel, which is a rare affection; the other a thickening from malignant deposit, which I am sorry to say is very common. Malignant stricture of the rectum has one feature that distinguishes it from malignant disease generally—viz., that it is often very slow in its progress. Its course is certainly sometimes rapid; but it may go on for twenty or thirty years before causing the death of the patient, as it always does in the end, in spite of remedies, unless he be carried off in some other way. Both of these forms of stricture exists in the same situation—viz., between two or three inches from the anus, where the rectum makes its turn backwards; and it may be said, that whenever stricture exists in the rectum, it is within reach of the finger. There is no rule without exception: and so it is possible that stricture may occur, in rare cases, higher up; but I never met with an instance of it during a practice

of thirty years, in which I have had pretty good opportunities for observing this class of diseases. It is quite true that it is often believed that stricture occurs at the distance of four or five inches from the anus; but this depends on a deception. A bougie passed into a healthy rectum goes up pretty easily till it reaches the promontory of the sacrum, when its progress is apt to be arrested; and the medical man, unless aware of this source of fallacy, may be led to suppose that a contraction of the bowel is the cause of obstruction. I might mention many instances of this; but one, which is particularly striking, will suffice. It took place in the case of an old lady, whom I was asked to see by Dr. Begbie; she had been between two and three years under the care of two deservedly-distinguished practitioners of this city, now deceased, on account of supposed stricture of the rectum. On entering her room, I saw a large tray covered with bougies of every possible size and consistence. On examination, however, I found all the bowel, within reach of my finger, sound; and, therefore, expressed a decided opinion, that no stricture was present. Some months afterwards, the patient died; and Dr. Begbie, and one of the other gentlemen, were present at the examination of the body; in which the retum, like the rest of the intestinal canal, was found free from contraction. The gentleman expressed great surprise, and said that not less than three hundred hours had been spent in introducing bougies, and how it was that no stricture existed he could not understand; so he proceeded to make an experiment by introducing the bougies in the same way as he had been accustomed to do during life, and found, that when it had reached the height of the supposed stricture, it was stopped by the promontory of the sacrum. When you consider the duration of the deception, and the high standing and eminent talent of the gentlemen thus deceived, you will allow that this case proves, in a very striking manner, the risk of being misled by this source of fallacy. But that any man can maintain the existence of a stricture at a still higher point—for example, twelve or thirteen inches up—or still more, profess to cure it, I say, is what I cannot understand any one capable of, unless he be extremely ignorant, or, still worse, intentionally deceiving the patient. Unhappily, however, it has long been a practice with specialists, in this class of diseases, to employ treatment directed to strictures alleged to exist in such situations. I will now show you a patient, in whom this has been done, although I firmly believe he never had anything wrong with his rectum.

[The patient, W. C., aged forty-two, residing at York, was brought in, and stated that ten years ago he suffered from some bowel complaint, accompanied with a mucous discharge from the rectum, and that he was then sent up to London to consult a certain rectum doctor, who told him that he had a stricture of the bowel, and passed very long bougies for a distance of about thirteen inches. He remained nine weeks under his care, the bougies being introduced about

four times a week. He was, however, no better, and returned to York with some medicine and a bougie which he had purchased in London by the doctor's direction, and which he has passed himself two or three times a week during the ten years that have since elapsed. The man having left the theatre, Mr. Syme said:—]

Had I told you such a tale as this myself, you might perhaps have supposed me to be romancing; but you have now heard it from the patient's own lips. But you may say that this is an old story, and that the light has perhaps shone in upon this practitioner during the last ten years, and shown him the error of his ways; but not long ago, I was consulted by a gentleman from England, who had been just before under the charge of the same person, and treated in the same way, although his rectum was as sound as any man's in Edinburgh. Such practices should be made fully known to the public as well as to the profession. It may be difficult to do this in London, from a natural unwillingness to incur the hostility of vindictive neighbours, but not being influenced by any such fear, I am resolved to do what may be in my power.—*Lancet, April* 7, 1855, *p.* 356.

166.—ON THE TREATMENT OF VARIX.

By Henry T. Chapman, Esq.

[The modifications of treatment by bandaging are considered under different heads.]

1. *Curative Treatment of Simple Uniform Varix.*—Whether it arise primarily or consecutively, the tendency to dilatation may readily be counteracted at an early period by equable and steady support in conjunction with cold and astringent lotions, although time will still be needed to restore the full tone and elasticity of its coats in order to guard againt a relapse. Unfortunately the surgeon is rarely consulted until the dilatation has become confirmed, and the duration of the treatment will, of course, depend very much upon the length of time it has existed.

Plaster strapping, as suggested by Mr. Hodgson, applied according to Mr. Scott's method from the toes upward, is not ill-suited to fulfil the end we must aim at,—that is to say, the closure of the vein to the circulation for a longer or shorter period,—if the requisite amount of pressure by such agency could be borne by the patient sufficiently long. After a while, however, the skin is sure to become heated, inflamed, and excoriated, and is sometimes deeply cut by the edges of the straps, whatever may be the plaster employed or the material on which it is spread. I have, accordingly, for some years entirely abandoned its use in the treatment both of ulcers and of varicose veins in favour of the following very simply constructed bandage, which possesses all the advantages of plaster strapping without its inconveniences:—

Placing the patient on a low seat, and elevating the foot until the veins empty themselves by the gravitation of the blood towards the trunk, I apply wet straps of linen or calico precisely in the same manner as Mr. Scott applied adhesive plaster. The bands for this purpose should be from two to three inches in width, and from twelve to sixteen in length, stout enough to prevent them tearing easily, but not too thick. Adjusting the middle of one of the shorter and narrower of these bands, previously soaked in water, just above the heel, the two ends are brought forward over the ancles, drawn tightly, and crossed upon the instep. The middle of another is placed beneath the sole of the foot, its extremities brought up firmly over the instep, and laid down smoothly upon the other. A third is applied, like the first, from behind forwards, but a little higher; and thus, ascending the leg, the process is repeated with the rest of the bands, each one in succession overlapping the upper half of that below it, until the limb is firmly and evenly cased to the knee. Over the straps a calico roller is carefully put on, the greatest attention being paid to its equable adjustment, so that the pressure may be uniformly distributed over the entire limb.

Through its texture the course of the varicose veins should be moistened several times daily with cold water, diluted Goulard lotion, or solutions of alum, sulphate of zinc, or chloride of lime. Whenever the bandage is taken off for renewal, the dilated veins should be subjected to brisk friction upwards with the hand for some time, and a douche of cold water poured over them, the foot being kept in an elevated position until it is re-applied. In winter, and when patients are elderly or delicate, the free application of cold water or lotions is not always safe, and this part of the treatment must of course be left to the discretion of the surgeon. It is better under such circumstances to confine the astringent influence of cold strictly to the vessels affected.

No one who has not tried this mode of bandaging can form an estimate of the power of adhesion possessed by the wet strapping, or the amount of steady even support it is capable of affording. In these respects—as I have elsewhere stated when advocating their employment in the treatment of ulcers and cutaneous eruptions on the leg—their action is in no degree inferior to that of plaster, at the same time that the softness of the material allows of its closer adaptation to the inequalities of the limb, and precludes all risk of cutting or excoriation, accidents so common when plaster strapping is made use of that its contact could never be borne long enough to produce any permanently good effect.

The wet straps are especially serviceable in compressing the bulk of the soft parts before the application of the roller, and they give the latter a much better hold than it can take of the bare skin, or even of plaster, thus materially diminishing the chance of the roller slipping. Their greater permeability, again, admits of the more efficient

use of lotions, and their cheapness, when compared with diachylon or any other kind of plaster is no slight recommendation. Unless displacement of its turns should occur earlier, or uneasiness be felt at any point, if the skin be sound the bandage need not be renewed before the fourth, fifth, or sixth day of its application; indeed I have frequently left it undisturbed for a fortnight or three weeks.

In cold weather a flannel roller is preferable to one of calico in all respects. It is quite as elastic as Churton's cotton-web bandage, supports the veins more firmly and comfortably, and is not liable to rope. In summer, or with patients who find both flannel and calico heating, I employ rollers of coarse mull muslin, which are light and cool, and give as much support as calico.

Although an elastic stocking employed alone may sometimes arrest the progress of varix in its earlier stages, it can never be regarded as a curative measure. In fact the heating properties of the material rather augment the tendency to dilatation; and as the ordinary pressure exercised by it but partially empties the veins of blood, lateral expansion in a saccular or serpentine form will often go on beneath the stocking. When combined, however, with wet strapping its efficiency is very considerably enhanced. But in order to derive the full advantage from this combination, the dimensions of the leg for the stocking must be taken after the wet strapping has been worn for some days, and the limb has been reduced a trifle below its natural size. Patients, accordingly, who are unable, or who will not take the trouble to bandage a varicose leg in the manner set forth above, may adopt the alternative of an elastic stocking drawn on over a casing of wet strapping.

2. *Curative Treatment of Saccular, Vesicular, and Serpentine Varix*—In cases of long standing and more aggravated character, where great tortuosity, saccular expansions of the main trunk and large branches,—numerous clusters often sprouting from these latter,—and vesicular enlargements of the cutaneous veins exist, the support distributed over the general surface of the limb by the wet straps and bandage alone, not entirely emptying the diseased veins, will do little more than palliate the complaint.

To direct the pressure, therefore, effectually upon those points where it is most called for, I have recourse to compresses of lint or spongio-piline in combination with the bandage, large enough to cover the chief clusters and each of the sacculated portions of the vein separately, and thick enough to close their channels. These compresses, moistened in cold water or astringent lotions, are placed on the most salient dilatations, and the straps and bandage carried over them. Many of the varicosities rapidly disappear; others require the maintenance of the compression for a longer period; and in serpentine varix, especially, the process of reduction is occasionally very tedious. In saccular expension and mulberry-like vesicular clusters in the skin

I seldom rely for the completion of the cure on the mere diversion of the circulation into other channels.

Taking advantage of the known tendency of the lining membrane of the veins to adhesive inflammation, I endeavour to obliterate all lateral pouches and vesicular protrusions through the skin by submitting them to a high degree of pressure, so as to maintain their opposite surfaces forcibly in contact; no very difficult matter when they happen to be situated over a bone; but when they lie upon the soft parts of a calf, as is most frequently the case, they recede beneath the pressure of the ordinary compresses, and require some more unyielding substance to close their cavities entirely. To meet this difficulty, I make use of the leather cones with which billiard cues are tipped, or smooth wooden or ivory buttons, choosing a size corresponding with that of the varicose pouch; and for the blue cutaneous vesicles, glass or porcelain shirt studs. The former are readily retained *in situ* by lint compresses and the bandage; the latter I insert into a small slit or button-hole in one of the wet straps, and carefully adjust it to the pit in the integument through which the vesicle has protruded; and by their aid I generally succeed, ere long, in effacing both large and small cells.

Where the saphena in the thigh, as well as the branches below the knee, are diseased, the pressure from within upon the walls of the latter will be more than doubled by the superadded weight of the column of blood in the former. Until this dilating force from above be neutralized, no rational expectation of reducing the enlarged veins to their natural size can be entertained. At the same time, therefore, that the several remedial measures described above are brought to bear upon the yielding branches, the dilated trunk ought also to be effectually supported. As, however, it is not possible, by any form of bandage, to exercise the same degree of equable compression upon the veins of the thigh, as on those of the leg, I never attempt to carry the wet straps and roller above the knee; but, leaving them to counteract all undue accumulation below, I trust to Mr. Startin's elastic riband, wound spirally round the thigh from the knee to the groin, to divide the column of blood in the dilated trunk, and take off its downward pressure upon the branches; and I have more than once had occasion to observe that by the time the veins of the leg are restored to a healthy condition, the dilatation of those of the thigh has likewise materially diminished.—*Med. Times and Gazette. May* 19, 1855, *p.* 486.

167.—*Employment of the Chlorate of Potash as a Topical Application.*—The chlorate was used in solution in the proportion of from one drachm and a-half to three drachms to one pint of water. He (Mr. Moore) had found it very useful in cases of indolent ulcer and phagedæna, in cleansing cancerous sores, and as an application to the mucous

membrane of the nose, mouth, and tongue in cases of ozæna and secondary ulceration. The author suggested that the beneficial effects of the application were probably due to its setting free oxygen, and proposed its use in some forms of dysentery, with affections of the lower bowel.

Dr. Mayo said he believed Mr. Stanley used the same remedy (scruple doses every four hours) for phagedenic syphilitic ulceration.

Mr. C. Hawkins generally combined it in such cases with tincture of myrrh. The remedy was very useful in cases of cancer, in removing the odour, independently of its effect upon the sore itself. Scruple doses he thought scarcely sufficient to administer internally.—*Med. Times and Gazette, May* 19, 1855, *p.* 504.

168.—*Treatment of Piles.*—In the out-patients' room at St. Bartholomew's, the other day, Mr. Wormald ordered for a middle-aged man, of spare habit and yellow complexion, long the subject of piles, the following medicines:—A pill consisting of two grains and a half of blue pill every night, a scruple of extract of taraxacum, dissolved in water, three times daily, and an injection of two grains of sulphate of iron to the ounce of water, to be used every morning. For the relief of the irritation caused by the tumours, and by the tendency to prolapse of the rectum which attended them, the man was directed to place just within the bowel, after every motion, a small plug of cotton wool, well oiled.—*Med. Times and Gazette, May* 19, 1855, *p.* 491.

169.—*On Colchicum in Neuralgia.*—In the out-patients' room at St. Thomas's Hospital, Dr. Peacock has recently had frequent occasion to remark upon the examples of neuralgia occurring seemingly in connexion with the rheumatic diathesis. In these the treatment by antiperiodics has always failed, and it has been requisite to employ either colchicum or iodide of potassium. In several quinine had been largely tried prior to admission, and without success, while the rapidity with which benefit ensued when the change in remedy was made, appeared to show conclusively the real nature of the disease. It is important to bear in mind, in the management of many of these so-called nervous diseases, that their true pathology may often be humoral, and that resort to eliminants is almost as frequently necessary as to nervine tonics and antiperiodics. An introductory treatment by purgatives, diuretics, or the specifics against gout and rheumatism, will, in fact, in the generality of cases, secure a much better action for the quinine afterwards. The employment, in the first place, of one or other of these, is, indeed, a matter almost of routine with many practical physicians.—*Med. Times and Gazette, May* 19, 1855, *p.* 490.

170.—*Iodine in Fibrous Tumours of the Uterus.*—Dr. West almost invariably orders for those of his patients at St. Bartholomew's who are the subjects of fibrous tumours of the uterus, a long course of one or other of the preparations of iodine. The following is the prescription which was ordered for a middle-aged woman, who applied with that disease on Saturday last:—

Potassii iodidi gr. i.; syrupi ferri iodidi ℥ xx.; aquæ carui ℥ss.
Ter die sumend.

Dr. West remarked at the time, that were the patient one in the higher ranks of life, she would be just the one likely to be benefited by being sent to drink the Kreuznach waters. In common with Dr. Rigby and other physicians, Dr. West entertains a high opinion of the value of the iodides in procuring the diminution of these tumours.—*Med. Times and Gazette, April* 28, 1855, *p.* 412.

171.—ON LARYNGOTOMY.

By John Erichsen, Esq.

[In commenting on a rapid case of laryngitis which Mr. Erichsen attended, and in which laryngotomy was performed, the following practical remarks are made:—]

This case illustrates the rapid progress of acute laryngitis, and also the importance of effecting a timely opening in the air-tube, in order to establish an aperture through which respiration can be carried on until the obstruction about the larynx has subsided. A question of considerable importance, however, arises as to the part of the windpipe that should be opened; whether, in fact, laryngotomy or tracheotomy should be performed. In my opinion, laryngotomy is the operation that should always be preferred, and this opinion is based on the following reasons:—

1. That as in laryngotomy the air-tube is always opened below the seat of obstruction, there can be no necessity to make an aperture further from the seat of disease. In laryngitis, whether that affection assume the acute or chronic character, the obstruction in breathing is in a great measure mechanical, and dependent upon the infiltration of the submucous cellular tissue of the larynx, and partly of the large plane of this tissue, which lies behind the box of the larynx, and which, by expanding, as it were, into the pharynx, obstructs deglutition, and afterwards, by the extension of this swelling and infiltration to the lips of the glottis and the interior of the larynx, causing an impediment to the entrance of air into the bronchi. But, as has been pointed out by Mr. Prescott Hewett, this submucous cellular tissue terminates at the true vocal cords, where the mucous membrane becomes directly applied to the subjacent fibrous structures, the swelling and consequent mechanical impediment is confined to the limits of the thyroid cartilage, and any opening made below this will clear the lowest limit of the disease, which is always accurately and almost mathematically bounded below by the vocal cords. Hence an aperture in the crico-thyroid membrane is quite as effectual as one in the trachea.

2. Laryngotomy is a far safer operation than tracheotomy. On this point I need scarcely dwell; a glance at the anatomy of the parts concerned will be sufficient to establish it. The crico-thyroid membrane is nearly subcutaneous, and no parts of importance can be wounded in opening it, if we except the small inferior laryngeal artery, which crosses it, and which might be cut across, but from which I have never seen any trouble arise. The trachea, on the contrary, is not only deeply seated, but covered by a large plexus of bloodvessels, which, when rendered turgid by the asphyxiated condition that exists when these operations are required, pours out a large quantity of dark blood, and thus seriously embarrasses and delays the surgeon at a time when the life of the patient depends on the speedy admission of air to his lungs.

3. Laryngotomy can be much more quickly performed than tracheotomy. This I look upon as an inestimable advantage in many of the cases requiring operation. A few seconds, more or less, being sufficient to turn the balance either in favour of life or of death. The rapidity with which laryngeal obstructions—partly mechanical and partly from spasm,—set in, is sometimes so great, more particularly when an acute inflammation supervenes on chronic disease of the larynx, that life may be extinguished before the surgeon has time to open the windpipe if he endeavours to do so by tracheotomy. In extreme cases, such as where the lungs have become slowly engorged, the action of the heart is already enfeebled, and a sudden spasm occurring at the glottis, will at once place the patient beyond recovery. But even though life appears for the moment extinct, it is the imperative duty of the surgeon to open the air-passages as speedily as possible, and to endeavour, by means of artificial respiration, to recall the flickering spark; and it is impossible to experience a greater satisfaction in the exercise of our profession, or to witness a greater triumph of art, than in thus successfully snatching a patient away from the very jaws of death.—*Lancet, April* 28, 1855, *p.* 427.

172.—*New Forceps for Extracting Bullets.*—At a meeting of the Medical Society of London, Mr. Lee exhibited a new pair of forceps, invented by Philp and Whicker, for the extraction of bullets. The improvement consisted simply in the forceps being formed by separate blades, on the plan of the midwifery forceps. By this contrivance less space was required for grasping the bullet, as the blades could be passed one at a time, and subsequently locked together.—*Lancet, Feb.* 10, 1855, *p.* 456.

173.—*Nævus treated by Ligature.*—Mr. Spencer Wells says, if a nævus be small, a ligature is sufficient; but, if large, the knot should be tied over a piece of bougie, which can be twisted each day after the operation, until the thread cuts its way through the base of the nævus.—*Med. Times and Gazette, Nov.* 4, 1854, *p.* 460.

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